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Luna B. Leopold

HYDROLOGY, GEOMORPHOLOGY, AND ENVIRONMENTAL POLICY:
U.S. GEOLOGICAL SURVEY, 1950-1972, AND UC BERKELEY, 1972-1987

With an Introduction by
Thomas Dunne

Interviews Conducted by
Ann Lage
in 1990, 1991

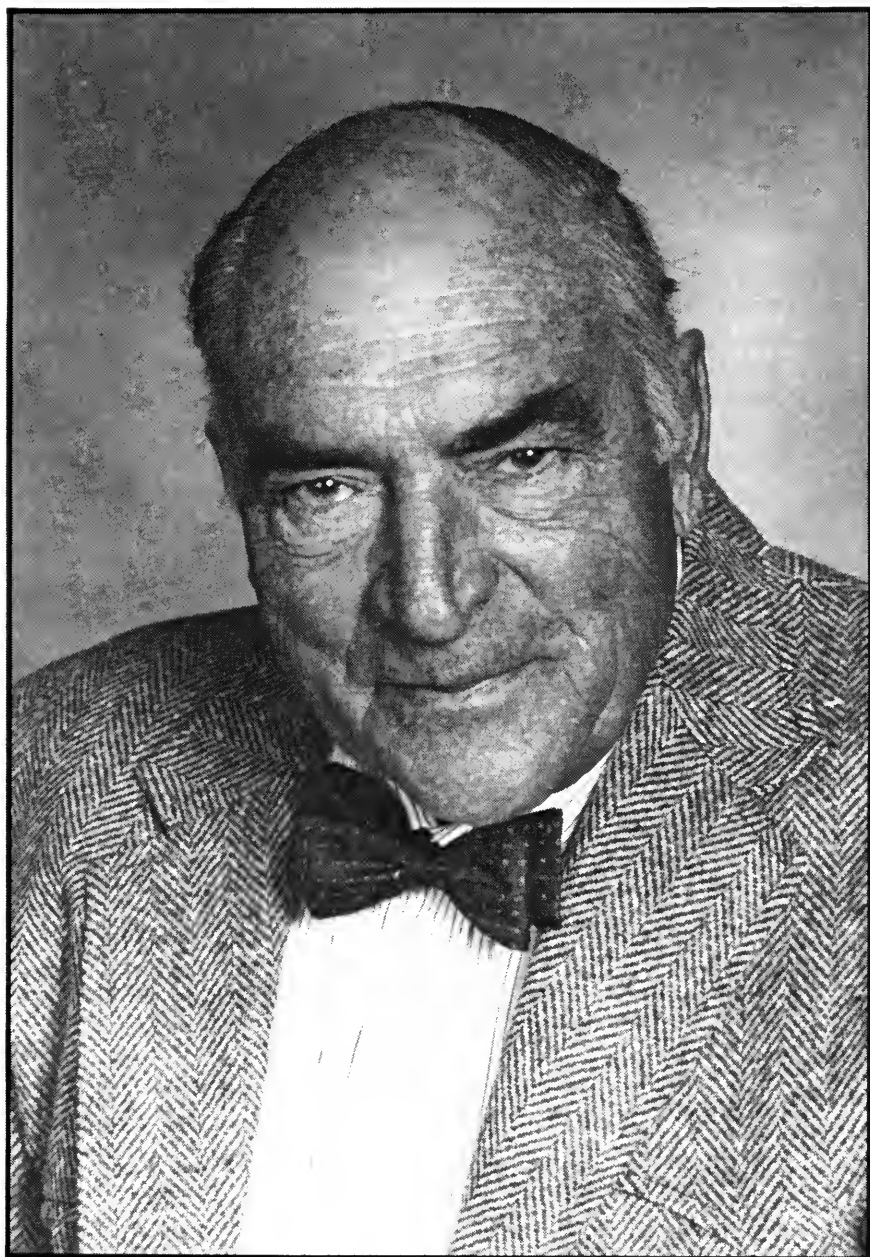
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Hydrologist, educator

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Family and youth in Albuquerque, New Mexico and Madison, Wisconsin: influence of father, Aldo Leopold, in development of scientific skills and land ethic; siblings Starker, Nina, Carl, and Estella; education, Wisconsin and Harvard, 1930s, 1950; jobs with Soil Conservation Service, Army Corps of Engineers, Bureau of Reclamation, Pineapple Research Institute (Hawaii), 1930s-1940s; chief hydrologist and research scientist, USGS Water Resources Division: administrative reorganization, personnel policies, publications, new programs of scientific research, field trips, relations with other government agencies; genesis of scientific papers in hydrology, geomorphology; colleagues Thomas Maddock, John Miller, Walter Langbein, Herb Skibitzke; environmental policies on Florida Everglades Jetport, Trans-Alaska oil pipeline, Colorado River issues, Hell's Canyon; Sierra Club Board of Directors, 1968-1971; teaching and research at UC Berkeley Departments of Geology and Landscape Architecture. Appended interview of USGS colleague David R. Dawdy.

Introduction by Thomas Dunne, Professor of Geological Sciences, University of Washington.

Interviewed 1990, 1991 by Ann Lage. The Regional Oral History Office, The Bancroft Library, University of California, Berkeley.

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University of California Water Resources Center

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INTRODUCTION--by Thomas Dunne

One day in late 1968, I was perched at the end of a long desk in Luna Leopold's Geological Survey office, making Einstein bedload calculations on a slide rule. Suddenly, into the room swept a group of men who settled in an arc around the other end of the desk. "Luna!" said one of them. "What are we going to do with Alaska?" I felt my neck contract slightly into my shoulders, in anticipation that a big issue was about to be discussed far above my head. It was a period in which Luna was pioneering the assessment of how the proposed Trans-Alaska oil pipeline would impact rivers and other wild resources. Such experiences became common throughout the months when I worked as Luna's research assistant during a lull in my graduate student career. The research group swirled between discussion of large environmental-policy issues and analysis of data collected and analyzed with our own hands. During the succeeding quarter century, while sharing with Luna many field trips in diverse lands, committee work, ideas for teaching, research and writing, I have watched this interplay of broad vision and hands-on experience exert remarkable influence on people and institutions.

A conversation with Luna Leopold is a vigorous experience--bracing for some, overwhelming for others. You're expected to get in there and dig. Think! What do you think? Here's what I think! The intense roving eyes, resonant voice, challenging questions mean that we're swimming out here in the deep water. This is not idle gossip, but meaty stuff about science, personal conduct, history, wild rivers, and politics. Luna brings the reticent into the conversation with courtly patience, and will listen to others with deep concentration. At other times his Stygian expression will flash into a grin, the tip of his tongue becomes visible between slightly open lips, his index finger will scythe the air in a loop from right to left as he has some new insight, recognizes an oversight or an absurdity, or imagines something new that "we ought to do". Luna thrives on lengthy, analytical discussion carried on for hours, or intermittently over years, or on a long field trip. Breakfast may extend for half a day.

I don't know another academic person with such a strong style as Luna's. Students sense in him something striking, different, even formidable. Some are unnerved, but others are drawn to him as a role model. Many decide to emulate him: his method of note-taking, of speaking, the way he thinks, the way he organizes himself and his life. He has a presence and an intensity. Reciprocally, he places great value on young people, encouraging them to search for the critical new idea that will move an entire field or institution to a new level of understanding or effectiveness.

Fundamental to Luna's style is the way he integrates his intellectual, aesthetic, and home life. The houses in Berkeley and Pinedale, Wyoming, have views chosen with great care; here it is difficult to forget mountains, rivers, the sea, wild game, and the need for their husbandry. There is a desk by the fire. A miscellany of books lie close at hand: Hurst's studies of the Nile streamflow, Darwin, Robert Graves, Alexander's military adventures, birds, a few collections of technical papers leather-bound by Luna's hand. The walls hold paintings of western American landscapes. There is good wine, the flame of a wood fire, an oil lamp or a candle reflected in it. Scientists, students, lawyers, administrators are hosted with great generosity and warmth by Luna and Barbara, whose ebullience leavens the proceedings and reminds everyone that the science, the careers, the plans, the programs are worth something only if they are humane. Here, Luna works on developing a perspective on a problem concerning science, environmental management, or public administration. When delivered, it is strongly cast. Whether right or wrong, he has worked hard at crafting it, sought extensive review from colleagues and students, and he keeps working on it. I have watched him mulling over problems for twenty-five years, and through his literature I can trace others for half a century.

The integration is evident also in the way Luna values individual craft. He enjoys doing his own river surveying, plotting his own graphs and maps for publication, carpentry, bookbinding, and building cabins for himself and others. When he took up the building of stone fireplaces, he read Ben Franklin's original paper on the design of fireplaces and stoves. The historical roots of a skill, an idea, a scientific development, are extremely important to him. He values also the crafts and music of others, as marks of their individuality. Hand-wrought objects collected on travels lie around within reach so that they can be picked up, turned over and marvelled at. "Gee! how d'you think they made that?" he'll say quietly.

There is a seriousness and intensity about Luna which is easy to mistake for competitiveness and self-absorption. He just doesn't choose to project silliness very often--though a glass of wine or a brand-new experience sometimes loosens him up. A guitar will often release another of his muses. He has written songs that express his love of doing science, environmental conservation, and the camaraderie of his River Boys team during the 1960-70s. Once, late in a day of maple-sap collecting in Vermont, Luna discovered a list of annual maple-syrup yields scribbled in pencil on the inside wall of the sugar house. He was excited when he recognized the possibility of correlating these yields with weather variations, and he spent much of the evening merrily standing in melting snow as he questioned the farmer about the sugaring process and factors that might cause yields to vary. On visits to Africa, his joyful enthusiasm for bird identification, animal ecology,

and learning about traditional herding practices persisted through days of hillslope surveying and bumpy rides. The only two times I have ever been charged uncomfortably close by a rhinoceros was in Luna's company. They just don't seem to like his concentrated stare!

The main public result of all this energy, of course, is a scientific career of international and inter-generational significance. Luna defined a field of research at the intersection of the traditional disciplines of geology, climatology, and terrestrial ecology, and was emphasizing the interacting roles of climatic change and human impact on land and water even during the 1940s, long before these issues were appreciated by most environmental scientists. More specifically, he yoked together surface-water hydrology and fluvial geomorphology and changed the latter into a quantitative science that provides a basis for environmental management and aesthetic appreciation of landscape. As chief hydrologist in the U.S. Geological Survey and later as a professor, Luna fostered a generation of young geoscientists in government and academe who have extended his quantitative interdisciplinary approach to other branches of earth-surface studies. When the need arose, Luna applied his process-based hydrology and geomorphology to the problem of environmental impact assessment in the cases of the proposed Everglades jetport and the Alaska pipeline. Though copied only badly in most cases, his examples still provide models for improvement in this vital activity that anticipates and seeks to minimize environmental degradation.

Throughout his career, Luna has emphasized the value of data and their honest use. He has pressed for government agencies to collect data and to disseminate them promptly and in a form that is transparent to citizens. He tirelessly stresses to young people the value of making one's own measurements in a backyard rain gauge, at regularly measured channel cross-sections, or simply from photo stations that can be re-occupied to document landscape change. They would be easily convinced if they had seen Luna in his nightshirt reading a stream gauge through a telescope from his deck and then using the resulting data as the basis of a scientific article on the effect of urban growth on floods. They would also be stimulated by his mischievous sense that out of such a simple measurement can come a scientific result with the power to help people understand and conserve the landscape that is so important to him.

It is difficult to imagine the development of surface-water hydrology and fluvial geomorphology without Luna Leopold's role as a restless, energetic leader of the U.S. Geological Survey's Water Resources Division, and later as professor of both geology and landscape architecture at the University of California. In these careers he has been a teacher *sensu lato* from the undergraduate level to the highest levels of government. He has challenged scientists to confront their responsibilities for Earth, and shown them how to develop the tools to undertake that task with a sense of optimism and deep appreciation. His

career illustrates this passion, rooted in his strong sense of history, ethical responsibility, and love of the Western American landscape.

Thomas Dunne
Professor of Geological Sciences
University of Washington

September 25, 1992
Seattle, Washington

INTERVIEW HISTORY--by Ann Lage

Luna Leopold, professor emeritus of geology and landscape architecture at the University and former chief hydrologist of the U.S. Geological Survey's Water Resources Division, was interviewed by the Regional Oral History Office as part of its extensive collection of oral histories in water resources. Leopold's oral history documents his life work as a hydrologist who redefined the field of hydrology; a geomorphologist who changed geomorphology from "an arm-waving pastime to a quantitative science;" and an administrator who transformed a government agency into a major research institution in water resources. It also chronicles his contributions as an educator in helping to shape university programs in hydrology and geomorphology, and as an environmentalist in bringing both science and ethics to bear on environmental problems.

Luna Leopold's early life and family background are of considerable interest. He is the second son of Aldo Leopold, pioneer in scientific wildlife management and author of A Sand County Almanac, a collection of reflective essays which stands alongside the works of Thoreau and John Muir as philosophic underpinnings for the modern environmental movement. Knowing that Aldo Leopold's life has been thoroughly documented, our discussions in this oral history focused on familial influences important in shaping Luna Leopold's scientific and ethical outlooks. Especially valuable are his recollections of his father's teaching by example the skills of precision craftsmanship and careful observation of nature; and his relationships with brothers Starker and Carl and sisters Nina and Estella, all of whom have followed distinguished scientific careers. Also important in understanding Leopold's interest in the lands and rivers of the Southwest are his recollections of his mother's family and his youthful experiences in New Mexico. Other areas of his background discussed here are his education and educational mentors at the University of Wisconsin and at Harvard and his early career experiences with the Soil Conservation Service, Army Corps of Engineers, Bureau of Reclamation, and the Pineapple Research Institute in Hawaii.

A significant portion of the oral history is devoted to Leopold's twenty-two year career with the United States Geological Survey, including his seminal research during this period, recollections of field trips and colleagues, and a perspective on the sometimes wrenching changes he instituted as chief of the Water Resources Division from 1957 to 1966. A videotaped interview with Luna Leopold was conducted in 1988

by R.C. Averett and W.W. Emmett for the USGS and was invaluable in preparing for this oral history. Mr. Leopold has made available a copy of the videotape for deposit in The Bancroft Library. To supplement his remarks and give another perspective on his leadership of the Water Resources Division, Leopold suggested that we interview his colleague, David Dawdy. That interview is included here as an appendix.

The final sections of the interview focus on Leopold's teaching at the University of California, his involvement in environmental issues, and insights into his major research work during more than fifty years in the field of hydrology. Throughout the oral history a picture emerges of a very gracious person, an intense scientist, sometimes intimidating in his strong sense of mission and his adherence to the highest standards in science and personal conduct. The conversations with Luna Leopold also reveal his sense of joy in intellectual inquiry, scientific discovery, and camaraderie with his fellow travelers along the research trails.

Mr. Leopold was interviewed in his office in the Department of Geology at the University. The first three sessions took place in May and June of 1990. Interviewing resumed in January 1991 after his yearly sojourn in Wyoming. The eighth and final session took place in May 1991. The interview transcripts were lightly edited in this office for clarity and continuity and reviewed by Mr. Leopold, who made only minor changes to his words. Some of his scientific papers have been placed in The Bancroft Library; his technical field notes have been given to the Geological Society of America. In his personal library he has an extraordinary collection of his personal journals from a lifetime of hunting trips and field trips, which he has hand bound in leather.

While this oral history was underway, in September of 1991, Mr. Leopold was awarded the National Medal of Science, the nation's highest scientific honor. Previously he received the Distinguished Service Award of the Department of the Interior (1958) and the Rockefeller Public Service Award (1971).

We are grateful to the Water Resources Center of the University of California and in particular to its director, Henry Vaux, Jr., for a major contribution to make this oral history possible. This is one of twelve Regional Oral History Office interviews funded in whole or in part by the Water Resources Center since 1964. The United States Geological Survey also contributed substantial funds to underwrite a careful documentation of Luna Leopold's role in that agency. Additional funding was received from the Departments of Geology and Landscape Architecture at the University of California, Berkeley.

The Regional Oral History Office was established in 1954 to record the lives of persons who have contributed significantly to the history of California and the West. The office is a division of The Bancroft Library and is under the direction of Willa K. Baum.

Ann Lage
Interviewer/editor

January 20, 1993
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Luna Bergere Leopold

Date of birth Oct 8 1915 Birthplace Albuquerque N.M.

Father's full name Aldo Leopold

Occupation Professor Birthplace Burlington Iowa

Mother's full name Estella Bergere Leopold

Occupation housewife Birthplace Los Lunas, NM

Your spouse Barbara Beck Leopold

Your children Bruce Carl Leopold, M.D.
Madelyn Denette Leopold Kazanski, Esq.

Where did you grow up? Madison Wisconsin

Present community Berkeley, Calif.

Education BS Civil Engineering, Wisconsin; M.A. Physics
Metorology, UCLA, PhD Harvard.

Occupation(s) professor

Areas of expertise hydrology, geomorphology

Other interests or activities

Organizations in which you are active many environmental
groups.

I EARLY LIFE AND FAMILY

[Interview 1: May 15, 1990]##

Memories of Duck Hunting in Albuquerque

Lage: We were going to start with family background, which is a natural thing, but in your case probably more important than with most people, and try to get a picture of what your family was like and how it influenced you, since the focus of this interview really is on you.

Leopold: It must be remembered that when I was a small child in Albuquerque, New Mexico, things were entirely different than we know now. I can remember very well, for example, when my father drove up down the alley with a brand-new Model T Ford, the kind with the brass front and the little acetylene lamps on the side, and one of those little black tops that folded back. We used to go hunting down near Los Lunas, where my family came from, at a place called Tomé, where many of our relatives are buried in the Tomé cemetery. Tomé is across the river from Los Lunas. My father had acquired a very small adobe one-room house that we used as a shack, and there was a nearby little pond where he went hunting.

That was a wonderful time of life because I can remember my father would hunt, go out on the little pond in the morning, and when we came back to Albuquerque that evening he'd have twenty-four, all drake mallards. In other words, there were so many ducks that he could choose to shoot only drakes. And of course, twenty-four was the limit in those days. Nobody considered that to be excessive, but clearly it was something quite different than what it would appear now.

Lage: And then were those ducks part of your meal?

Leopold: Oh, of course.

Driving a Model T Ford, the tires were awful, and I can remember in the evening one time having a flat tire between Albuquerque and Los Lunas, only eighteen miles, but it was dark and there were no flashlights. So, since the road went by the railroad track, parallel to the railroad track, my father and I would--and I forget who else was with us--would sit on the running board of the Model T Ford and wait until the train came. When the train came, in the headlights of the train, in that few moments that the train was going by we would work desperately to change the tire. Blow up the tire. You know, you had to put a patch on it. Blow up the tire in the dark.

Lage: In the few moments of headlight, that wasn't much time.

Leopold: And then we had to wait for the next train.

Mother's Family: Sheep Ranchers in the Southwest

Lage: What do you remember of your mother's family? Were you close with them? Did you see them a lot?

Leopold: Yes, because as it turned out, over the years I was kind of the favorite grandchild, so I spent a lot of time, more time than practically any of the other children, with the family. See, the family had moved from Los Lunas, where my mother was born, to Santa Fe, and I'm not quite sure what the date was, but it was probably around the turn of the century, I suppose. I know that the family moved into a house which was down Grant Avenue just a block away from what we called the big house, the house that I knew. I suppose they were renting it, because the house that I knew when I was growing up was nearer the plaza, and it had been built by the army in 1846 when General Doniphan came into Santa Fe at the beginning of the Mexican War in August of 1846. That was at that time that the army formed Fort Marcy, which is up in the hill above Santa Fe. They built a series of sort of look-alike houses for the officers' quarters.

My great-grandmother--my grandmother's mother--had acquired one of these houses. I suppose it was about the turn of the century. Later, and I'm not sure what the sequence was, but my great-grandmother gave this house to my grandmother and her family. I suppose that the house was enlarged somewhat later, but it was an adobe house with walls about two and a half feet thick. Right behind the kitchen, outside, there's a little line of very small, all connected buildings, one of which was an outhouse. So in those days they had an outside privy. And they

had a peach orchard. It was only two blocks from the center of town, so it was quite a different kind of situation than you have now, with all the urbanization.

Lage: Santa Fe's changed tremendously.

Leopold: Oh, my God, yes. Tremendously.

So, yes, I spent a lot of time in Santa Fe, primarily at the time that I was in early high school.

Lage: You'd go down for summers?

Leopold: I went down for summers, and then when I became a senior in high school, my father told me that now I had to go and get a job. It was expected that you work. So that summer, the summer of, I guess it must have been my senior high school year, I got my first job, non-paying, but anyhow, I spent the summer someplace else.

Lage: Do you have more to say about this side of your family and how they might have had an impact on you? Was it kind of two different worlds that you were exposed to there?

Leopold: No, I don't think so. I'm not sure just what kind of a question you're asking. What is it that you'd like to know about that?

Lage: In terms of values, attitude towards the wilderness and the land, even religion.

Leopold: They were sheep ranchers, that family, and had been sheep ranchers for generations. It is said that my great-grandfather, Antonio José Luna after whom I'm named, was sort of the--not only the patrón, but he was also the hidalgo of Valencia County. They were very important politically, and they thought they were going to lose a certain election, so Antonio José Luna arrived at the polls with a Winchester across his saddle, and he said, "All my sheep are citizens, and they're all going to vote." So he put the sheep through the place where the votes were counted, and of course he won.

I'm told that relative to other people, although they ran a very large bunch of sheep, they probably were better managers than many others. Although when they were running, let's just guess at 15,000 sheep, the country must have been pretty badly grazed. Later, when the sheep of the family were cut down to about three thousand head, the land probably was being kept better.

Lage: Were these things that were talked about, or did you notice them at the time?

Leopold: No. As a matter of fact, the other problem was that they were very-- I'm not sure just what the word is, but I never was taken to the ranch until after I was out of college. What happened was that all the grandsons, or the nephews, that couldn't get through college, who were dropouts, they were taken to the ranch and they tried to make them into sheep men. Well, if they couldn't get through college, they certainly weren't going to be very good sheep men. None of them lasted more than a few months. But those of us who might have really had the sort of background to run the place were never invited. As a matter of fact, I was never invited at all. I simply went there one time after I was out of college.

And then when the family ranch was to be sold--remember, this was my grandmother's ranch--her first sons were the ones who were running the ranch after her brother Solomon Luna died.

Lage: So your mother's brothers were running the ranch.

Leopold: Yes. My mother's brothers were running the ranch. First Eduardo and then later Manuel. When Eduardo died, I was probably in the last year in high school. His younger brother Manuel took over the ranch and ran the ranch successfully for a good many years. The ranch had been set up as a trust by his mother, my grandmother. It was for the purpose of supporting all these women, you see. There were thirteen children in that family, and eleven of them were women. Therefore, the whole purpose of the trust was to see to it that the women in the family always had someplace to come to.

About the time I was out of college--I'm jumping, but this is an important matter for what you're asking about--I could see that my Uncle Manuel--my mother's brother--was getting not only too old but too tired to keep running the ranch, and they decided that they were going to sell it. The people that really ran it were the brother and his sister, my Aunt Nina, who was the matriarch of the family, and her brother Manuel. They were discussing the fact that they were going to sell the ranch.

Now, the ranch was a pretty extensive place. It was down near Mogollon, in the Gila. They owned, apparently, about one section, as I understand it. Six hundred and forty acres. They controlled about thirty-five sections, so they controlled thirty-five square miles that extended from the Gila Basin north into the San Agustín plains.

Well, this heritage I had now seen once. I had gone there to see it once. So my older brother Starker and I--he at that time was already a professor at the University of California--we made up our mind that we didn't want that land sold; we wanted to protect it, and we decided we would make an offer. So I went to my aunt, and I said to her, "My brother Starker and I would like the opportunity to run the ranch rather than have you sell it." I explained that we were both college educated, we knew something about agriculture. I was an engineer, he was a forester, we knew a lot about land. She looked at me and she said, "You don't know a damn thing about sheep." I said, "No, but I can learn." "No," she said, "that's impossible, because you never worked under Solomon Luna."

So they sold it, under very unfavorable conditions because they wouldn't listen to any advice. They paid about three-quarters of their earnings in taxes, so they ended up with nothing like what they should have had. And then, of course, within two years, the ranch, which had been bought by a bunch of Texans, was sold for four times the price or something like that. Anyhow, that was what was happening.

Lage: Why did she object to your trying it, you and Starker?

Leopold: That was what I'm trying to explain. This family felt that the only way to do things was the way it had always been done. I remember when the Soil Conservation Service was formed, and on all the surrounding ranches--big ranches in that part of the country--the government was coming in and paying through the CCC [Civilian Conservation Corps] for drilling wells and doing fencing and improving the grass, and erosion control. My Uncle Manuel would have none of it: "I don't want any goddamn government people on my land."

But they were very interesting people. They were real characters. Eduardo, my mother's eldest half-brother, Eduardo Otero, was, as his sister Nina was, red haired and very light colored. They were all great gamblers, all of them. They loved to play poker, they gambled about everything. The story goes that Eduardo was playing one of the gambling games, apparently, in northern Mexico--one of the west Mexican towns--and apparently he had amassed a very large number of chips. He heard the director of the gambling place come up to the dealer and say in Spanish, "Don't let that damn gringo get any more money." Eduardo apparently--you can imagine, light colored and blue eyed and red haired--spoke out in perfect Spanish, said, "Thank you very much; I'm just about through anyhow," and he pulled all his chips together and left. [laughter]

The Lunas and Bergéres

Lage: So their heritage was Spanish, from Spain.

Leopold: Oh, yes. It starts...Well, if you look in the Bible in which I have put as much of the history of the family as I could get, the first record of the Lunas was one of the great figures in the history of Spain. He was called the condestabel, or constable. He was really was the most potent political man in Spain under the king. His name was Alvaro de Luna. He was beheaded by the king in 1432. We know a lot about him. And then I have traced all of the family. There's a big break in the history in the Middle Ages that I don't know very much about.

But they came to New Mexico from Spain, the way I figure it, about 1680. The leader of the expedition was Diego de Luna. When he came up from Mexico into New Mexico along the Rio Grande, he formed two towns. One he called after his cousin, the Duke of Albuquerque; he called it Albuquerque. The other he named for his own family; he called it Los Lunas. So that we know then that the family settled there in the Rio Grande Valley. Because the railroad happened to have a main way station in Albuquerque, it turned out to be a large city, and Los Lunas is just still a little town. We know quite a lot about that history. But they were all sheep men.

Lage: Had any of the family married outside the cultural setting before your mother did? Here she married a government man.

Leopold: Until my grandmother, I think essentially not. After my grandmother's first husband, Manuel Otero, was murdered, she married my grandfather Bergére, who came from England.

Lage: Was he English?

Leopold: He was a professional pianist, apparently a very good one. We got to know something about this just last summer when Barbara and I were in England. We got in touch with a long-lost relative of ours who happens to be my second cousin. His name is Michael Berger. Apparently, he and his family have been very interested in tracing the genealogy of that part of the family, and from what we all can conjecture, my grandfather--my mother's father--was sent away from home, from Liverpool, because apparently his stepmother couldn't get along with the boys. So they were each given a couple of thousand pounds and told to get out.

My grandfather came to New York in the hope of studying under a great piano teacher there. Our conjecture is that thinking it would be better as a pianist not to have the name Berger, he added an "e" to it and called himself Bergère. That is our conjecture; we don't know. But then he was turned down by this great teacher, so he really left the piano and didn't come back to it for many, many, years. Went to New Mexico where he met my grandmother, and that's where the family started.

But the previous generation before that marriage, all of the people in the Luna family had married people from New Mexico, of Spanish origin. The red hair and blue eyes come from people in Castile.

Lage: Was there any problem with melding these two cultures together-- your mother's and your father's?

Leopold: That's a little hard to say. There were so many of them that I didn't know very well. The one that I knew best was the matriarch, my Aunt Nina Otero. She was a very strong-willed woman, very proud of her origin, very smart. She was in politics and in business and in a whole lot of other things. She was the one that turned us down when we wanted to run the ranch. But she was also very pecuniary, and she and her sisters tended to look down on my father because he was an intellectual; he was not a businessman who made a lot of money. But that's a passing conjecture of mine; at least that has been my experience.

Lage: It wasn't something that you saw functioning in the family?

Leopold: No, it's simply that when something came up, and I had heard--. She never said anything to me about it, but it was quite clear that they thought my father, who, of course, became the most famous of all of them, wasn't really amounting to very much because he didn't make a lot of money.

Lage: Right. And did the fact that he worked for the government--? Your remark about the family's attitude toward the government--

Leopold: No, that had nothing to do with it.

Lage: That didn't bother them.

Leopold: It was a question of the fact that he didn't earn a lot of money. Because all the rest of these people were business people, you see.

Lage: Maybe your argument that you and Starker had gone to college and therefore you could run the ranch didn't hold too much water with her.

Leopold: Oh, no.

Move to Wisconsin: Family Interest in Archery and Craftsmanship

Lage: It sounds like an interesting background to come from.

Leopold: Yes, but of course I saw that only in summertime. Much more important in the long run was my association with my father's family. Or my father alone.

Lage: Let's look at that aspect, then.

Leopold: My father was a very farsighted man when he lived in New Mexico. For example, he was secretary of the Chamber of Commerce; he was very influential in an organization of sportsmen that he either formed or participated in. That's all written up; there are a lot of books about that. But the important part was that he was very successful in his civic and in his government work, and was advanced to higher and higher jobs until they offered him this opportunity to go to Wisconsin, where he was to become the associate director of the Forest Products Laboratory. But as I say, there's no use going into that because there are books written around it. A lot is known about that.

Lage: We should concentrate on how it might have influenced you.

Leopold: But when we moved to Wisconsin, when I was about eight, my mother found it very difficult to get settled there because she missed the sunshine, and the dark, long winter days of cloudy weather she found very difficult to get along with.

Lage: And just the change in the landscape and being away from her family must have been hard.

Leopold: But my mother was a very remarkable woman. She became an extremely good botanist. She could name any darn plant that you ever grew. She was very good at birds. She was a good gardener. And how in the world she got along with five children on that little money, I don't know, but when my father died in 1948 he was earning \$6,000 a year. And my mother--. We all went to college. We all got along quite well.

Lage: She does sound remarkable.

Leopold: Oh, she was. And then she was, of course, the state champion in archery. We went into archery when I was about ten. And this becomes one of the most important things of my life. When my father got interested in bows and arrows, and he started to make bows and arrows himself, this was really the beginning of the whole business of handmade, first-class articles of beauty as well as utility.

Lage: So this became kind of a family--

Leopold: Oh, yes. And everybody, all of the family then, at least the elder children, primarily my older brother and I, were shown that to make things by hand of great beauty with a high degree of perfection was the thing to do. So we all became highly skilled craftsmen.

Lage: What kinds of things did you make?

Leopold: I not only made my bows and arrows, but I made knives, quivers, jewel boxes, on and on and on. But that was a very important thing. And all with hand tools. In other words, we didn't have a machine tool in the house. Everything was done by hand.

Lage: Were these things expressed in words or just by example, the value of making something by hand? Would this be talked about as well as done?

Leopold: No. But for example, when I started making knives, I can remember spending a large amount of time, evening after evening, going over drawings of knives with my father, talking about every little nuance of how the shape should be. I mean, he took a great interest in the whole question of "make it beautiful and do it with perfection." So these things that were turned out were things of great beauty, I can tell you.

Lage: Do you still have these things?

Leopold: My best knife that I made then, I have used all my life. So I certainly do have it. The greatest piece of archery tackle that my father made, I think to our knowledge no one has ever achieved anything like what he did. I don't know whether you realize that when you shoot a bow, your arrow goes from your chin and therefore it's pointing upward when you look across it. Therefore, when you talk about pointblank at a certain distance, it means that the angle between the arrowhead and the bottom of the chin is just enough rise in elevation to go a certain distance. My father had a set of arrows, had a bow that shot

them, that shot pointblank at a hundred yards. No one to our knowledge had ever achieved that before. The most beautiful set of equipment that I've ever seen in my life.

Lage: Did a great deal of study go into the dynamics of the arrow?

Leopold: Yes, my father was a friend of a physicist who wrote several books on bows, a man that later I knew in my professional life. So that although my father himself was not versed in the physics of projectiles, he certainly knew people who took a lot of interest in that. So there was a lot of back and forth. My father interested a lot of other people in Madison in making bows. And in making knives, as a matter of fact. Several friends of ours got interested in making knives when I started making knives. But they were older people, people that were a generation older than I.

Lage: Did Starker make things like this also?

Leopold: Beautiful. Oh, Starker was a superb craftsman. He not only made good bows and arrows, but he made fly rods, for example. He tied the most beautiful trout flies I've ever seen in my life. He was a real expert on that. He carved a perfectly beautiful chest for his daughter. I don't know who's got that chest now, but he was a woodcarver. Well, in every way, Starker was a first-rate craftsman in practically everything.

Lage: Did your mother also--

Leopold: My mother shot the bows.

Lage: [laughs] She was the expert at that.

Leopold: She was state champion for more than a decade. She was a national champion in one aspect of archery. She was the best archer that we had ever seen. She never was beaten; she just stopped shooting bows and arrows, and somebody else--. In other words, as far as the women were concerned, she was the best archer that ever existed in Wisconsin. She was very good.

Lage: So she took all these things up with enthusiasm.

Leopold: Oh, my goodness, yes. Oh, yes. I should say so. She was superb.

Lage: What got your father interested in this? Was it a new way of hunting, or was it the craftsmanship angle?

Leopold: Oh, it was both, because you see-- . Well, you are acquainted with the things that have been written about his life. He was a great hunter, as I told you. And then as we moved to Madison, it became more and more clear to him as he became more involved in what was later to be called ecology--he named it, really--he began to see that it was more important to be more related to the study of rather than taking of game.

Passing on Family Values and Traditions##

Leopold: But all of that has been written up in great detail.

Lage: Would he talk to you boys about this as you were growing up?

Leopold: Oh, you didn't have to talk about it; you just did it, that's all. So basically, although he never really gave up shotguns, we really did less and less hunting, and more and more shooting with a bow. Some of the greatest experiences that we ever had were going deer hunting with a bow, long before anybody else was doing that. We never killed a deer.

Lage: You never made--

Leopold: Oh, no, never did it. But it was very exciting to be shooting. Nowadays you see people buy the tackle at the sporting goods store. We didn't. We made everything ourselves.

But I can remember my father saying to me one time, "One cannot grow up to be a gentleman without some experience with dogs, guns, and horses." So after my first summer with a paying job, when I was in early college, I came back from that full summer work, and my father said to me-- . And that's when he was unemployed. We were living off his savings, and we were really very poorly off. He said, "How much money did you earn?" I said, "I came back with \$90." He said, "That's fine. What are you going to do with it?" I said, "Ninety dollars will pay for a whole year of my tuition and books at the university, and I'm going to spend it on that." He said, "I don't think I'd do that." I said, "Why?" He said, "Why don't you buy yourself a shotgun?" I'd been shooting all my life this little 20 gauge--a single-barrel shotgun. A very nice little shotgun but very cheap. So I then got hold of the catalogue of the finest shotgun maker in the world and ordered a made-to-order shotgun. I was fourteen, and that gun which I-- . Oh, yes, and my father said-- . I said, "Dad, I've only got \$90, and the shotgun costs \$120." He

said, "I'll give you the rest." So that shotgun is now worth, what, \$5,000. It's a perfectly wonderful weapon.

So anyhow, he was very interested in having the right equipment. And to keep care of equipment; he was very, very particular about keeping care of axes and shovels and shotguns, things like that. So nobody spoke about these things; you just did them. You watched your father sharpen a knife, and you sharpened a knife that way. You watched him clean his gun, and you cleaned your gun that way. So you don't talk about these things; you just did them. It was a marvelous way to be taught, because when you watched a really great craftsman go to all the trouble to do it absolutely right, then what the son does is to follow the same thing. You do it right, you do it the best you possibly can, and are not satisfied with anything else.

Lage: It doesn't always seem to work that way, though, in families, that the tradition is passed on that way so successfully.

Leopold: Well, I'm going to give an example, because it doesn't pass on necessarily either easily or surely. My father was, of course, very interested in birds but never made a big deal out of it; he just knew birds. Well, I wasn't. I thought birds were, except for shooting, birds weren't very interesting. But when I had my first job after I left college, I bought a pair of very good binoculars, and from that day on I became a birder. The difference was that I had never seen a bird through a pair of glasses. No one ever said, "You should do that," but once you got into it, once you got the right equipment, then all of a sudden I've been a birder ever since.

Lage: And they're habits of mind that seem to be passed down, of being very thorough. This is evident in looking at your journals from the early years.

Leopold: Well, I keep a journal because my father kept a journal.

Lage: Right.

Leopold: And I write small in the journal because my father wrote small in the journal.

Lage: And you put the initials of the people who were on each trip at beginning of each entry.

Leopold: That's the way he did it. That's the way I did it.

Lage: Was Starker like that also? Did he pattern himself in that way?

Leopold: Quite, yes. Now, our journals are really quite different, but, oh, yes, he has a--

Lage: But he did keep one too.

Leopold: I'm not sure exactly how he kept his journal. But yes, he was a very good notetaker.

Reading and Religion

Lage: What other kinds of activities do you remember from these boyhood years? Was reading something important in the family?

Leopold: Oh, my goodness, yes. My father always had trouble with his eyes. Every night, without fail, my father would sit in the living room with his eyes partly closed, and my mother would read to him. They were the best-read people I've ever seen. They read everything. They read plays, they read novels, they read history, they read new books, they read classics. I never saw such a well-read family, very well-read people.

Lage: And then would you listen in on this?

Leopold: No, because ordinarily I was studying. But it was a pattern. I look back at the things that I read, and I'm just amazed at how much I read. No, we read a lot. Everybody in the family read.

Lage: Were there boyhood books that you recall as having particular excitement for you, or influence?

Leopold: Yes, I was crazy about Robert Louis Stevenson. I read an awful lot of classical material because I found it interesting. And then later on this grew into an interest in historical novels. But it was simply part of the family business that reading was a very important matter.

Lage: Not like having a television today.

Leopold: No, that's one of the really great difficulties with television, is that you're not going to get the same kind of education that you get from reading books.

Lage: Right. Or family setting, really.

- Leopold: Yes. Of course, even in those days there weren't very many families that read the way my family did. The idea of my mother reading to father every single night, without fail.
- Lage: Did your mother follow her Catholic upbringing?
- Leopold: Oh, that was wonderful. We were all brought up Catholic.
- Lage: Oh, you were?
- Leopold: Oh, yes. All of the children, all five of us, successively, one after the other, dropped out of the Catholic church at about the same time. About the same age.
- Lage: About what age was that?
- Leopold: About ten or eleven. I found that the little neighborhood Catholic church that we went to at Madison, all I can remember is they talked about money, how the church needed money. I found it very uninspiring, uninteresting, and as a matter of fact, just a waste of time. My mother went to church every Sunday, but when we'd go hunting on the weekend, my mother would say, "God will know that it's more important for you to go out a day with your father than to go to Mass, so you go with your father." She went to Mass, but that was the way we were taught.
- Lage: Did she object when you all successively dropped out of the church?
- Leopold: No. Oh, I think she was sorry in a way, but any verbal objection, no. As long as we were going out with our father, doing something that was interesting outdoors, that was more important.
- Lage: But she didn't mind when you completely gave up on the church as a religion?
- Leopold: If she did, she didn't talk much about it.
- Lage: Did your father ever express any opinion about it?
- Leopold: No. My father was so well read, he knew more about the Bible than I ever knew. He never went to church, but he knew a lot about religion. Like many things, he didn't say what you should or shouldn't do; he simply encouraged you to do what you're going to do. So there was really very little discussion about that.
- Lage: And then have you followed any organized religion since then?

Leopold: No. My first wife insisted that my children be brought up in the Episcopal church, and indeed my son, who is now a physician, went to an Episcopal school, a very good school, a private school, and I think got a lot out of it, but has not followed up religion since. Although our daughter, I think, has now turned to one of the churches, I think an Episcopal church, and apparently gets considerable satisfaction out of it.

Father's Family in Burlington, Iowa

Lage: Did you have many ties to your father's family in Burlington?

Leopold: Yes, but I never spent as much time there as I spent in New Mexico. You see, what happened in New Mexico was that not only did I spend several summers there during high school time, but then I went there to live after I graduated, and I worked in New Mexico. And I've worked in New Mexico all my life. So that I saw a lot more of my mother's family than my father's family. But the Leopolds were a very remarkable group of people. Very remarkable people. Again, it's all been discussed in books, but among the things that were important were that all of my father's family--his sister, his two brothers--were extremely good golfers.

Lage: Oh. I've never seen that in the books.

Leopold: No. Father was not. And I never played golf. Starker apparently played golf for a while, but the idea of being a sportsman in the Leopold family was always an important thing. These people were good. My Uncle Frederic was shooting the same score as his age when he was seventy-five.

Lage: That's very remarkable.

Leopold: That same Uncle Frederic, the youngest brother of my father's family, became the world expert on one kind of bird, the wood duck. Everything that's known about wood ducks, he really pioneered. He put up boxes in the family yard and followed the life history of these little ducks, and wrote about them extensively, and lectured about them, and a lot is known now about wood ducks, and he started all that.

The family, of course, made fine wood furniture. This is a Leopold desk [in Luna Leopold's office]. All during the Depression, all during the war, no union ever unionized that plant. No one was ever fired. They had a very old factory, but

they turned out beautiful stuff with very happy personnel, apparently. The unions tried to unionize it, but apparently the two brothers, Frederic and Carl, were very advanced in dealing with employees. They had some kind of a benefit system of bonuses that depended upon output, so without being a sweatshop, it was very successful in dealing with employees. Very loyal people, all the employees.

You'll still see these desks. The bank I go to down in Berkeley is full of these Leopold desks. They don't know where they came from.

Lage: You can tell by the style?

Leopold: Yes, I can tell by the style.

Lage: Is the business still operating?

Leopold: No, my uncle sold the business in one of these takeovers. He had turned the thing from a struggling little shop into a very successful small business and sold it for a very high price. But sold in one of these takeover jobs.

Lage: But some time ago.

Leopold: Oh, yes, it must have been, I suppose about twenty years ago, I suppose.

Skate Sailing, Skiing, and Hunting in Wisconsin

Lage: What other outdoor experiences do you remember? I've run across references to your skate sailing, and I saw in your journals some skiing trips.

Leopold: In those days, you see, before the days of ski lifts, my best friend Bert Gallistel, who's one of my closest friends still; his father was the chief engineer for the University of Wisconsin in Madison. He and I did everything together. We went to high school together, we went to college together, and we taught ourselves to ski. But since we worked all day in school, we started to ski at night. So we would leave after studying; we would start out at nine o'clock in the evening on our skis, and we taught ourselves to ski in the dark.

Lage: That's pretty hard to do.

Leopold: We nearly killed ourselves a lot of times, too. But we learned a lot about skiing and skate sailing.

Lage: This was more cross-country-type skiing, I would assume.

Leopold: We didn't know anything about so-called downhill skiing, because there were no slopes. Everything was fluffy snow, and therefore you learned to telemark before you learned to christy, you see.

Lage: Did you learn these various techniques?

Leopold: We taught ourselves just doing it, that's all. And then there was skate sailing. Growing up in Madison was an amazing experience because here you had all these lakes. We hoped to skate on Thanksgiving Day every year. We'd go to the smallest lake, Lake Wingra, and we would skate across this thin ice before it was ready. [laughs] Why we never fell in, I don't know. But then when the big lake, when Lake Mendota became frozen--and there were years in which it froze when there was no wind and no snow, and it was just a glass all the way across, four miles across. Bert and I one time timed ourselves sixty-five miles an hour over a mile course on our skates. It was a very exciting thing to do. We could never make up our mind whether we liked skate sailing or skiing better.

Lage: What exactly was skate sailing?

Leopold: The skate sails Bert and I built were of very unorthodox design. Mine consisted of a T-shaped spar, the base eight feet, the height fifteen feet. On this was stretched a triangle of muslin cloth. The apex of the triangle dragged on the ice. The long spar sat on your shoulder about three feet from the base of the T. To move, the pressure of the long spar against your shoulder pushed you along. To tack, the whole sail was lifted over your head and put on the opposite shoulder. We could tack about thirty degrees off the wind. Because of the force components we could sail faster than the wind pushing us, so a forty mile-per-hour wind could push us sixty miles per hour, approximately.

Lage: Sounds like a great, exciting boyhood.

Leopold: Well, then, of course, we did a lot of hunting too. Bert and I did not, but I mean my family did.

Lage: Would these be weekend trips, or longer trips?

Leopold: No, mostly just a day or two days, weekend trips.

- Lage: What do you think--. A lot of conservation-minded people now think hunting's a terrible thing. How do you feel hunting developed your own sense of sort of the ecology? It doesn't do it for all hunters, but it surely seemed to for your family.
- Leopold: The main thing about hunting is hunting is an exercise in sportsmanship. The idea of killing a lot of something is simply not the way it's supposed to be.
- Lage: And it wasn't the way it was in your family.
- Leopold: No, absolutely not. But as I say, my father had gradually changed his mind. He finally gave up hunting more or less completely in his later years.
- Lage: Altogether?
- Leopold: No, it was simply that he was more interested in doing other things. Now, for example, after we had the shack and spent practically every weekend up there, we were trying to build up the population of whatever animals we had. But when I came back during the war, when I came back for a visit when I was still in the army, we'd go up to the shack, and my father would say, "Why don't we go out and see whether we can find you a duck?" He wasn't interested in shooting, but he was interested in having me find a duck.

So the family turned more and more to growing plants and trying to do what my sister now has done very well--to learn how to restore prairies in their original form with all the original species. We tried for many years to try to grow a population of quail in our land. That's way in the far northern edge of quail territory, because they would simply kill off in the winter, in the big, cold winters. But my father never really gave up hunting. He simply was interested more in doing other things. But when the boys came back and if they wanted to hunt, why, that's fine.

But your question about hunting in general, well, it's like this business of animal fur. You can carry all these things to extremes. If you're going to say you can't wear furs, then you shouldn't wear shoes, or you shouldn't eat meat, or you shouldn't kill cattle for beef. I mean, there are extremes that people go to, and I would say hunting is one of the most important things I ever did and I have no intention of giving it up, and I think there's nothing more important as far as learning sportsmanship than to go hunting. Because you learn all of the things that are needed: how to take care of equipment, how to treat other people, how to deal with landowners, how to deal with fences, and

respecting other people's property, and then how to treat animals and birds.

Yes, there are people who in the name of conservation don't like hunting, but there's an awful lot of very good conservationists who do like hunting. I say if you look at the aspects that I'm speaking of, hunting as we knew it was probably the most educational experience that we did, because it involved all of these ethical types of problems, so that you learn to do it the right way. And besides, develop a skill.

Lage: And be in the outdoors.

Leopold: Yes, that's right.

Developing Habits of Close Observation of Nature

Lage: What about the habit of very close observation of nature? That's something that the family seemed to have had across the board. Did that come through your hunting experience?

Leopold: Clearly it came straight from my father, no question about that. But then there were other aspects of it. I studied a lot of botany, but I was never a very good botanist. Because I was primarily in another field, my knowledge of taxonomy never really improved very much. Many times when I came back after I'd graduated, came back to be for a weekend with the family, and we would go up to the shack. The general thing was you went for a walk, and my father would look at a certain plant, and he would say, "Of course you'll remember that this plant is called so-and-so." Well, hell, I didn't remember it, but he would never embarrass you by suggesting that you didn't know. But then he would remind you. And then we could talk about that plant.

But close observation was the basis of his teaching, too. I'll never forget the final exam in his course when I took it. The final exam consisted of a little sketch, and I remember it still. The sketch was a cross-section. He didn't tell you where it was. It showed a road, and a fence, and a rock, and a dead rabbit. That's all the cross-section showed. The question started like this: Where is this location? Where did the rock come from? Why is the rabbit dead? What would be the relationship of the rabbit to the road? To the roadside, and on and on and on.

You had to think of all these relationships. You had to be in Wisconsin because the rock was exotic. The rock was rounded and therefore it was moved by something, and you had to think, what could have moved the rock? Why was the rock sitting there alone? What killed the rabbit? Why the road? Because the road bank was protected, you see, from farming, and therefore it had some plants in it that you wouldn't have found across the fence. The reason the rabbit was there, because the plants were there.

Lage: I see. Would these be the kinds of questions that your father put forth when you were out together, too? Would he encourage you to notice things in that way?

Leopold: No, he would look at something and then he would start talking about why it was so. So he wasn't as much asking questions as discussing with you why you thought a certain thing that you observed was true. He always listened to what you had to say, even though you may not have had very good ideas about it.

Lage: Did writing the journals help you observe more closely, do you think?

Leopold: Yes, I think so, because the reason that a journal is important is that you'd be surprised how fast you lose something, how fast you forget. If you sit down as we always did and wrote your journal that night, everything's fresh in your mind and you'll see things in the freshness of your memory that you would have lost had you not done so.

Lage: But it seems to me it would also tend to make you, during the day, more aware, more conscious of what's going on.

Leopold: Possibly. I never thought about it that way, but that's possibly true.

The unfortunate thing is that my father never lived long enough to see how successful this teaching was and how the children had all responded to it. I think he would have been tremendously pleased.

Lage: But he had some indication of what direction you were going in. At least the older--

Leopold: It was too early, actually.

II EDUCATIONAL AND EARLY CAREER EXPERIENCES

Early Schooling in Albuquerque and Madison

- Lage: One of the books that I looked at said you'd entered college at age fifteen. Was that unusual at that time?
- Leopold: Actually, I must have been fourteen. It was very peculiar. In the first place, when we lived in Albuquerque, my mother sent me to school when I just turned five. We went to a little private school run by two very talented women who had probably--I don't think there were more than twenty students. The learning was such that you made two grades per year. Or maybe it was a grade and a half. But I do know that when we left Albuquerque, I was eight years old when I was in sixth grade, and I was too darn young, because the problem was that by the time I got up at that level, I wasn't remembering as much as I should.
- Lage: So you went right to sixth grade in Wisconsin? They accepted the placement?
- Leopold: Yes, but my problem was at eighth grade. I was going to a grammar school in Madison, and the teacher was very nice. She didn't insist that I learn the arithmetic that I should have known, so that when I got to high school, I was not as good in arithmetic as I should have been, and I didn't like geometry, which later I came to love, and I had simply fallen behind. So when I entered college and started the engineering school, there was a six-weeks' period during which you prepared yourself after you entered college. At the end of six weeks you were to take an examination to see whether you could stay in school. I learned all my high school mathematics in six weeks, but the problem is, you don't learn it well enough. So compared with the students that we see here, my math has always been very much less than I would like.
- Lage: Has that been a problem in your field, or has it directed you in a way that you might not have gone?

Leopold: It simply has not turned out to be my particular specialty. Observation is my specialty, and analysis. And I wish that I knew more math, yes.

Civil Engineering at the University of Wisconsin:
Influence of Professor Von Hagan

Lage: You started in civil engineering. What were you thinking of as a field then?

Leopold: The way I went into engineering was interesting. I told you that the father of my best friend, Bert Gallistel, was a mining engineer who became the superintendent of buildings and grounds at the university. We'd gone through high school together and learned to ski and skate sail together. So when school started, we were walking up the hill to go to register. Bert said, "What are you going to register as?" I said, "I don't know. What are you going to be?" He said, "I'm going to go into engineering." I said, "I think I will too." [laughter] So we walked together, and we signed up, and he said, "I'm going to get in this line; this is mining engineering." I said, "I don't think I'll be as interested in that as civil engineering; I'll take the next line." So I went into civil engineering.

Well, then you come to the most important thing that ever happened to me, is that the head of the civil engineering department at the University of Wisconsin was a man by the name of Professor Leslie Von Hagan. Von Hagan happened to be the father of another close friend, Charles Von Hagan. Bert and I and Charlie were very close.

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Leopold: Professor Von Hagan was a very strict disciplinarian. He did a lot of things that no university professor before or since has ever done. Everything that he did, I have done in my teaching. For the first three years we hated him, he was so tough on us. But here's what he did. For all the civil engineers, he gave a course in engineering English. The course was taught every semester, and every civil engineer had to take a course every semester for all the years in college, in English.

Lage: In writing, basically?

Leopold: Both. Every week a student had to turn in to him twenty words. The twenty words were supposed to have been words you picked up during your reading. Of course, you didn't have time to read, so you went to the library and you took out a dictionary and you wrote twenty words. You had to define them, and then he would choose from all these words that were turned in to him, and he would give them to you in an exam, and say, "Define these words."

Lage: So you had to know your words plus the other words.

Leopold: And you sure learned a lot. Boy, I'll tell you. I'm greatly influenced by what that man taught me.

Lage: Now, have you done that? In your teaching?

Leopold: Not just that, but for example: everything that you turned in to him had to be bound in a particular way, in exactly the same way every time, in a manila folder, exactly the way he wanted it done. You never threw any notes away. You did your computations on the side so he could check them. And then if you got a problem wrong, you got the same problems handed back to you, and you did it again and again and again until it was perfect. And then if it still wasn't done right, the same problem was handed to you on the final examination. After you finished the examination, here are the problems that you haven't finished. If you didn't pass them, you were flunked.

Lage: What did you do if you needed help with that problem? Obviously, it didn't come easy if you kept getting it back all semester.

Leopold: The difficulties were along these lines. In those days you didn't have calculators, and everything had to be done to three significant figures, for example, and therefore we used logarithms. But you had to use six-place logarithms, so that if you didn't copy the logarithm number down correctly, the six letters, you're going to get something wrong, you see--

Lage: You had to be pretty precise.

Leopold: --and therefore you might make a slight error, and you had to do that over. Not very often--. The problems were discussed in class, so that there was no reason why you certainly couldn't get it on the second or third time. But if you didn't do it perfectly, you'd get it again and again. So that was a very important influence. This man was--. He was wonderful.

Lage: Did he have influence on other students as well?

Leopold: Oh, yes, there's no question about it. But I think I was probably more influenced than most.

When we went to summer camp, we had six weeks of intense work.

Lage: And this is still civil engineering?

Leopold: Yes. We went up to the Baraboo Hills where the summer camp was held. When we were laying out the railroad that we had to design, and Professor Von Hagan was out with us in the field, we found that he really was a human being, that he was very funny, was honest, very friendly, and we all just loved the hell out of him. Whereas formerly, you thought in class he always seemed so gruff, and so rough on you. But he was very influential on everybody.

So that training was absolutely wonderful because of the discipline that this particular professor gave you. There were not very many other things I could say about that training, but engineering is always a wonderful thing to study because you're thrown up against a lot of different kinds of problems. But I think that everybody who went through that particular university system got something that no one else has ever gotten. He was just beyond belief.

Lage: Were there other professors that you recall in a similar way?

Leopold: At Harvard, yes. My professor at Harvard was very important to me. But other than-- Yes, there was. One of the young instructors in civil engineering later became a professor and then became the dean, then became the head of all of engineering at Wisconsin. He was very important to me because he followed the same line of approach to students. Very demanding, had to be done exactly right, and when you look back, you just loved him for it. This was Professor Kurt Wendt.

Lage: Now, you say you carried this on into your teaching.

Leopold: Yes.

Lage: Has it been successful? Have you gotten the students to--

Leopold: Well, it's a different way, but there are-- See all those folders there?

Lage: Yes.

Leopold: Those are students' works that were handed in the same way that Professor Von Hagan made us hand them in. If they weren't stapled correctly, the student gets them back. For example, in my teaching, I wasn't very loved for this, but I told the class, when you hand in a piece of work in my class, if you have a mistake in spelling, I said, it's ten points off. I said, "I never correct a word that's a misspelled word without myself looking into the dictionary to make sure that I'm correcting it correctly. If I can look into the dictionary, so can you." Boy, when they started to get ten points off per spelling, my students paid some attention, I'll tell you.

Lage: It must have been unusual--at least, in my conception of engineering now--to put this emphasis on writing and on reading outside the field.

Leopold: I have absolutely no respect for this university, California, in the engineering school. None, because everything's mathematics and computers, and I don't think that's engineering. Not the way I know it.

Designing a Broadened Field of Study, with Lasting Impact

Lage: Did you switch out of engineering and into geology while you were at Wisconsin, or did you graduate in civil engineering?

Leopold: Well, what happened was that I found civil engineering to be much too constrained.

Lage: When did you decide that?

Leopold: About the end of my first year in college. There were no electives. I think in four years of college, I would have two electives. So I went to the dean, and I said, "I would like to make an agreement with the university that if I take five years instead of four to get my degree, I want to be able to study botany and ecology and plant physiology and geology and soils and agricultural climatology. If I take five years, would you give me just a little more flexibility in my schedule?" They said, yes, so that's what I did.

Lage: That was quite an overview. There seems to be a big change from your first entry into college and the casual way you decided to take engineering. And then a year later you had this kind of broad overview of what you wanted to study. How did you design a

series of subjects that you were going to take to round out your education?

Leopold: I wanted to know something about geology and biology, and that's what engineering was not giving me. So I took a lot of extra geology, and that was very important because I'd gotten interested in the one required course in geology. The required course in geology was taught by a famous professor at Wisconsin by the name of Warren Mead. I was so crazy about that course, it was just wonderful. One of the great teachers I studied under. So I started to take quite a few more courses in geology. And then I went over to botany and started more or less at the beginning with elementary botany and then advanced botany and then taxonomy, then ecology and plant physiology, and on and on. So that I came out with a considerable knowledge--training, not knowledge--training in the biological sciences, which most engineers don't get.

Lage: Was this anything that your father encouraged, or you just--

Leopold: Oh, he encouraged it, but this was really my idea. My father thought I was very foolish to stay in engineering. He said, "Why don't you go into something else?" I said, "I now believe that in order to talk to engineers I have to be an engineer." I said, "I want a degree in engineering in order to deal with engineers."

Lage: You saw them as a group that had to be dealt with?

Leopold: Yes. Because I could see what was happening in his profession, that there were a lot of sort of practical people who couldn't see that ecology had much to do with them.

Lage: I see. So you want to be able to talk their language.

Leopold: Yes. And it's been very helpful to me. Very helpful to me. I could see, for example, by watching my father, that you're not going to get very far in science of the kind that we were interested in without knowing something about biology. Engineering was not enough. So although my father hardly was directing this, he certainly was encouraging it.

Lage: I know you just kind of stumbled into engineering originally, from what you told me, but did you come to see it as something that maybe was missing from your father's background? Did it give you something that would be able to take you in a different direction?

Leopold: Well, I never thought about it as my father missing it, but I certainly was gaining something from it, because, for example, I

could think in terms of the physical forces, the kind of thing that we studied in physics and in structures and in bridge design, foundations, which in biologic training you simply don't get any--you never get any of it. It was a very good combination. As a matter of fact, the combination is really quite necessary if you're going to go into hydrology in the modern sense.

Lage: Were you able to take that broad base of studies at Wisconsin that you requested?

Leopold: Oh, yes, indeed. Yes. As a matter of fact, that's standard business now. But I had to fight for it in those days. They didn't believe in it at all.

For example, when I was teaching in this department [Department of Geology], I would say to graduate students, "You've come to the University of California, Berkeley, which is a very large and a very diverse place." I said, "I don't care what you do, but get yourself educated. Take what you want, but come out an educated person, because you can do so at Berkeley. There are no requirements as far as I'm concerned. Now, I expect you to learn some geology, but I'm not telling you what part of geology you have to learn. Be educated." That's what I told students when I was an advisor here--in other words when I was a chief advisor to students--because I believe that you're never going to learn everything, and that the individual ought to have a great opportunity to decide what combinations of things he wants to learn.

Lage: So you wouldn't be one of the educators who feels that there should be a core curriculum that everybody participates in?

Leopold: I think a person has to be educated in a broad way, but whether that is the way to accomplish that purpose, I don't know. I have a great empathy for a student who wishes to decide for himself or herself what kind of an education that he or she wishes. I think we should both give students an opportunity to do so, and to encourage them to do so, and give them some advice as they go. But if you graduate in geology from this department, as I said, I expect you to know some geology. But how you're going to learn that geology is up to you. But you're going to have to know something about these various subjects, all of which are geologically oriented. To come out, for example, without ever having taken a course in paleontology, which most of these students don't, I think is a shame. They don't know any biology at all.

Lage: It doesn't give them the kind of broad view that you've brought to it.

A Learning Experience at Coon Valley with the Soil Erosion Service

Lage: You worked with the Soil Erosion Service during the summers? Is that the non-paying summer jobs you referred to?

Leopold: Well, first I worked for the Forest Service at a forest experiment station. Then I worked for the Soil Erosion Service in a non-paying job at one of the experiment stations. The third summer, I worked for the Soil Conservation Service as a young engineer.

Lage: Was the Coon Valley experience one of those?

Leopold: I spent a summer at Coon Valley before the big experiment station was expanded into a big deal. When Coon Valley was first being set up, I was there as a non-paid helper actually laying out the experimental plots in areas that were later to be used by the experiment station. It was just getting started at Coon Valley at that time.

Lage: Was that an experience that shaped you in any way, or developed your interest in soils?

Leopold: Yes, some things that happened there were very important. Yes, I'll tell you one of the things, which was always a source of great embarrassment to me. I was a civil engineer, and I had just finished a course in surveying. Years later, surveying became one of the most important things that I do. I was running a transit, laying out experimental plots. Apparently I went in for lunch, a thunderstorm broke, and my expensive instrument was out there in the rain. I ran out and I picked up the instrument and I took it in the barn, and I started to dry it out. By the time I got it dried out and had looked through it, I had destroyed the spider-hair crosshairs, which of course could not be fixed except in the factory. I was fifteen.

Lage: Oh, you were very young then.

Leopold: Yes. I just turned fifteen. So I went to the head boss, and I said, "Sir, I've made a terrible mistake. I've ruined our instrument." And I can tell you, that was very difficult to do, but it was also a very great learning experience, because to

force myself to go and admit that I'd made a mistake, and to go to the boss and tell him immediately and in detail what I had done wrong--and of course, when you do that, there isn't very much the poor gentleman could say except to say, "We'll have to send it to the company to be fixed." But that was a moment of great growth, I'll tell you, when you forced yourself to say you had made a terrible mistake; and to admit it immediately and publicly, that was tough. I think that's one thing I remember the most about that summer.

Lage: That's quite a learning experience. How did you get interested in soil erosion and end up at Coon Valley?

Leopold: Well, because this was from my father's influence. We were interested in conservation, and I happened to be leaning toward the whole manner of how land was treated, and the one organization that dealt with that matter was the Soil Erosion Service. So that's where I started.

Thoughts on Breadth in Education and the Value of Field Experience

Lage: I think we've probably come to a good stopping point, unless there's something that comes to mind about the things we've been talking about that you think we should add.

Leopold: To summarize that part of our experiences, it was quite clear that an education demands breadth, and breadth you're not going to get in many of the ways in which certain courses or certain things are taught, such as engineering. Breadth also means reading, which my family did a lot of. When I first went to graduate school, you certainly did a lot of reading, which is not now required of anybody. Therefore, people are growing up without breadth without having read anything, and have usually not been forced to write very much, and therefore they find the whole matter of writing very difficult.

So the whole business of education has been turned upside down by the lack of experience in writing and reading, too much emphasis on computing and what are now called "models." That's a very bad turn of events, where you don't ever have to go out and see anything in the field; you construct something in your mind that you can put on a computer. Now people are being trained without any field experience whatsoever.

Lage: It's like they were empty vessels, if they don't have the field experience or the reading.

Leopold: That's right. And that's what's happening in engineering now. In this university here, civil engineering in the graduate school requires only that you learn computers and mathematics, and that's all.

Lage: No work in the field.

Leopold: No. Matter of fact, they resent working in the field, or do not encourage the students to work in the field. I know, because some of my student friends have gone there at my suggestion, and I turn and find out that this is not an education. And yet for some reason or another, Berkeley engineering, Berkeley civil engineering, is considered one of the best in the country. I don't believe it. That's not my idea of engineering.

Lage: Do you think other programs are similar, though? Maybe it's a trend not just at Berkeley.

Leopold: Unfortunately, this is not just this university. I went to a meeting a couple of months ago, of the Institute for Hydrology, American Institute for Hydrology, and there was a lot of discussion about training. I found, in talking to a lot of people, there's only one school that I've found out about that has the kind of education in hydrology that I think is a real education. It happens to be the School of Mines in Colorado. Not civil engineering, but the School of Mines. That's the only place that I've seen where I would consider that they are offering a real education in hydrology, because it has all the things that I've been talking about.

Lage: How about in geology? Is there an emphasis there also on kind of the "black box" approach? Computing and modeling? Or do they still have the fieldwork?

Leopold: That's a very touchy point in this department. It is indeed.

Lage: With you on one side and others on another?

Leopold: Yes. I'm not the only one on my side, but there's a real schism here. There is a difference of opinion as to the value of field work in the science of geology. Some laboratory scientists and theoreticians believe field work for students in geology is a waste of time. I feel field experience is essential. But in the last few years this has changed. Many of our more recently hired teachers are very good field geologists, and so the pendulum is now swinging back, I'm glad to say.

Lessons in Supervision at the Soil Conservation Service

[Interview 2: May 30, 1990]##

Lage: You wanted to start out today with some learning experiences at the Soil Conservation Service.

Leopold: In 1936, when I graduated from the University of Wisconsin, I had taken five years to take my degree because I was dissatisfied with engineering. At the end of five years I was probably the only person at that time trained specifically for work in the field of soil conservation. I took a job, a temporary job with the Soil Conservation Service in New Mexico, where I had many roots. I was paid \$77 a month working for the regional office in Albuquerque, and I was put in a reconnaissance survey team--not surveying, but resource surveys.

Leopold: There was a geologist in the office, Dr. Parry Reiche, that I got to be very close to, a man who was very important to me in my career. I had taken some geology, but I certainly could hardly be called a geologist; I was an engineer. Also, his secretary in the office was a girl that I started to go out with, a very lovely girl. One time she said to me, "Why haven't you taken any of those fine jobs that were offered to you? You have been working here for \$77 a month as a temporary. You don't even have a classification." I said, "I never heard about them." She said, "These letters are coming in, and they were never sent to you?" I said, "No." "Well," she said, "your boss apparently is simply pocketing them and doesn't let you see them." Then I began to realize that people can take advantage of you. It never occurred to me that people would take advantage of you. So that made quite an impression on me, that that's no way to handle young people.

At that time since I was a fledgling geologist, my geology friend, Dr. Reiche, had told me about the whole question of the effect of changing climate on the environment. I began to read the geologic literature about this, particularly written by Professor Kirk Bryan at Harvard. I began to see that there were people who just didn't believe in what we were doing. The Soil Conservation Service had one idea, but here were the other people, very important people like the professor at Harvard, who thought that we were crazy.

Lage: In what aspect of what you were doing?

Leopold: He said, "Man is not the cause of your erosion problems. Climate is the cause of your erosion problem." So I had long discussions with my geologist friend. Finally, I decided I wanted to learn something about this. So I went to the big boss and I said, "There are people who disagree with us. I suggest you send me to graduate school to study under Kirk Bryan and I'll come back and tell you, or tell everybody here, what this man is talking about. He's a very well-known man." They said, "Oh, he doesn't know anything. No, we won't send you to school." So I said, "I quit."

So at that time, Dr. Reiche had written a letter to Kirk Bryan at Harvard and said, "This young man wants to come and study geology under you. He wants to know your ideas, and I would suggest that you give him some help." And then Kirk Bryan, whom I'd never met, wrote to my father and said, "Your young man wants to come and work under me." I saw the letter later on. "I've always wanted to have an engineer come and study geology." He said, "I will give him a small scholarship, but that's only a small part of what it takes to go to Harvard, and I suggest that you help him out." But I was accepted to Harvard.

So I went to my father and I told him about this. Yes, he had gotten the letter from Professor Bryan. My father said, "Very well," he said, "I will give you"--and this is now when he was unemployed--he said, "I will give you \$900." It was costing at that time about \$2,500 to go to Harvard. So I had less than half of what other people had, but I was delighted to have it. So off I went.

Well, that's another whole story. But at the end of the year I didn't have any money, and there was no way to get any. There were no such thing as grants, you see. So I went back to work. This time, when I went back to New Mexico, having thought about my experience with this prior boss and having learned practically nothing--I was a very dumb engineer--I said, "I want to work with the man I've heard about"--I had never met him--"who works in Safford, Arizona. His name is Thomas Maddock, Jr." At that time Maddock was just being transferred to Albuquerque, so I had the opportunity to work under this man, about ten years older than I.

Tom Maddock has been one of my closest friends ever since. We have shared an office together, and Tom is an engineer. Tom grew up in Arizona, a very broad-gauge man, and he would put his feet up on the desk opposite me and he would be reading all the scientific journals. He would say to me, "Here's something we ought to do. You compute this." So under his guidance, I computed day after day after day, and by the time I finished

working under him after the first year we were probably as far advanced in the hydrologic sciences as anybody in the United States, because Tom read all the time and I tried the things out and ran my slide rule and computed for him.

Then, in contrast to this previous supervisor, everything that was good, Tom sent me to. Somebody would say, "We need a man to come to Washington and do such-and-such." Tom would say, "My assistant will go." And then he would get the money and he would send me. So I was sent on field trips, I was sent on conferences, and I was given every single opportunity that was possible. I learned a lot.

I learned something about what it was like to be a supervisor. Take care of your people. Assume that they are going to work hard for you and they're going to work hard for themselves and they're going to learn something, and that's the way to get ahead. You don't get ahead by keeping people down. You don't get ahead by putting a lid on them. You get ahead by helping them move ahead.

Well, that affected me all my life, because later on when I became a supervisor, then I did the same thing.

Lage: It's an unusual quality, I think.

Leopold: Yes. And of course, it has paid off again and again. In order to promote yourself you promote the people that work under you. And I mean promote in an intellectual sense; I'm not talking necessarily about promoting in a job.

Those were very important things that happened to me. Then, for example, in that same first year when I was working close to but not with this geologist friend of mine, I went on field trips with him.

Lage: With Tom Maddock?

Leopold: No, this was with Parry Reiche before I met Tom Maddock.

I was going with a girl that I was crazy about for many, many years. Her mother had some mining claims, and one of them was a claim in the mountains in the Jemez Mountains, and she asked me as a young fledgling geologist to go take a look at this claim, that happened to be a claim for kaolin, a clay. So I went there and it was an amazing geologic formation. I studied the thing and made a map of it. So I started to write a paper about this. I wrote this manuscript, and Dr. Reiche helped me. He told me this and he told me that and he guided me, and here was

my first published paper, a paper in geology. ["Climatic character of the interval between the Jurassic and Cretaceous in New Mexico and Arizona": Journal of Geology, v. 51 No. 1, pp. 56-62.] Without Dr. Reiche I never could have completed it. But I learned a lot by being under somebody who was really willing to help you. He furnished technical information as well as other kinds of advice. So again, the way to make things move is to help people. He certainly was a wonderful help to me.

Interestingly, shortly after that Dr. Reiche was sent to some other place, the war came along, I lost complete track of him for many, many years, and I never could really get a chance to thank him for all he's done for me. Last year a friend of mine here in Berkeley said, "Do you remember what happened to Parry Reiche?" I said, "No." "He's living here in town." I said, "He is? I haven't seen him for forty years." So I wrote him a letter, and I said, "I want to tell you after all these forty years, that everything in my career has been due to the help that you gave me." He wrote me a letter back that said, "That's very interesting." He said, "I've helped a lot of people, but you're the only person who ever thanked me."

Lage: Maybe they'd lost him too.

Leopold: I don't know.

Lage: Did you go to see him then?

Leopold: He didn't want to see me. He was an older man, and--. I don't know. But he was a very, very fine geologist, and I always felt that I had to do something to tell this man who helped me so much; that I had to tell him about how much I appreciated him.

Flood Control Surveys with Tom Maddock. SCS. 1938-1941

Lage: When you first went to the Soil Conservation Service, did you go as a hydrologist?

Leopold: No, I went as an engineer. I didn't become a hydrologist until after I joined the Geological Survey many years later. Although I had taken hydraulics, hydrology, as it turned out, when I started my work in the Soil Conservation Service I found that--. Looking back at it, I certainly didn't know anything. But under Maddock I learned a lot. I really learned a lot.

Lage: Now, what was the job with the Soil Conservation Service?

Leopold: I was simply called junior engineer.

Lage: And you were with the Soil Conservation Service from 1938 to 1941, three years.

Leopold: Yes.

Lage: What kinds of things were you assigned to do?

Leopold: Maddock and I were working on flood control surveys. Now, flood control surveys involve all the kinds of things that are in modern hydrology--that is, rainfall characteristics, infiltration, the translation of rainfall into the runoff hydrograph, the routing of the runoff hydrograph to some downstream point, the whole question of measurement in streams--everything that's in modern hydrology. But at that time it was not well codified, and at the end of that period of time, I think we both realized that we were as far ahead as anybody in the country, that we were learning an awful lot. Because we were reading the literature, and we were actually working out problems.

So what did I do, for example? I took it upon myself to chase rainstorms, and when a big flood occurred I would go rushing down to see the effect of the floods and to collect data on the rainfall that caused the flood, and I would make measurements of the highwater marks.

Lage: Your later jobs seemed to give you a lot of freedom to decide what the important matters to pursue were. Is that the case with this?

Leopold: Tom Maddock, as I told you, was a very, very intelligent supervisor. Since we worked very closely together, he gave me a lot of openings to--. Because I was very active. When I said, "Let's go chase that storm," he's say, "Go ahead. You'll get some data." So it was very different from many offices, where you have set things you were going to do. We were doing research, as a matter of fact. We were trying to develop new ideas.

Lage: Was publication part of the function of your job?

Leopold: As it turned out, I was the one that was interested in publication. Tom wasn't. Later on he became interested, but I published several papers during that period. But I didn't realize at that time how important that was going to become.

- Lage: In the USGS video¹ you made a reference to new hydrological principles developed by these soil conservationists-hydrologists. Could you expand on that?
- Leopold: Yes, because right at the end of the--just before the war, there were two or three figures that stood out as contributors of really new ideas. They included the famous engineer Robert E. Horton, the engineer W.W. Horner, and a man by the name of Sherman. What they were all working on in one form or another was the procedure by which you could compute the volume and timing of runoff from a precipitation event. Robert E. Horton was at that time working on what later became known as his famous infiltration theory, and Sherman and Horner were working on what is now the basis of most hydrology, which is called the unit hydrograph. When I was sent to Washington on one assignment by Tom Maddock, I was sent to work on data relating rainfall, infiltration, and runoff. We were generally supervised by the great Horton.
- Lage: Was that important in your development of your ideas?
- Leopold: Yes, because I could see then that Tom Maddock and I were indeed working on the right things. We were very close to having ideas similar to these great names that were working on this problem.
- Lage: But independently.
- Leopold: Right.
- Lage: Were you part of a hydrological division or anything like that?
- Leopold: No, Tom Maddock and I were simply the hydrologists on the flood control surveys.
- Lage: I see. And what was Horton? Was he in Washington?
- Leopold: No, Horton was a consultant to the Soil Conservation Service and the Forest Service in Washington. A part-time consultant. So the point is that at that time, we were independently doing--. Looking back at it, it was research, but we weren't supposed to research. But we were. We were on the right track.

¹Interview of Luna Leopold by R. C. Averett and W. W. Emmett, August 1988, for the U.S. Geological Survey. A copy is in The Bancroft Library.

Graduate Study at Harvard, 1937: Classical Ideas in Science

Leopold: The year at Harvard in 1937 was extremely important. I had never heard anyone use the word "science." Kirk Bryan was always talking about science, meaning the intellectual growth in scientific thought. No one had ever talked to me about science. All of a sudden I began to see that that was a very important matter, to provide new information to other people by learning new things. That's what science is all about.

And there was a difference from the present age. In those days a graduate student was expected to read a lot, and that has simply been lost. Completely lost. Now what people do is read short articles in current journals. But when I went to graduate school, what you were being taught was not the new things but the classical ideas.

Lage: And that's not the case anymore?

Leopold: Absolutely not.

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Leopold: The classical ideas. For example, William Morris Davis was even at that time beginning to be seen not as the great tower of knowledge in physical geography, but he was seen at that time as somebody who contributed a lot but we had to move ahead into quantitative geology. But Kirk Bryan insisted that we read all kinds of essays written by the great man. But each time, he was saying, "We must do it a little differently; we must proceed beyond this."

We read all the classical people in the field of physical geography. At that time there was a great discussion among the most advanced thinkers in physical geography about the difference in view between the great William Morris Davis from the United States, and Walter Penck from Germany.

Furthermore, Professor Bryan expected you to know languages. The first paper he gave me to read was in Spanish. The next paper he gave me to read was in German, and boy, I'll tell you, that was tough, because I knew very little about languages.

Lage: Did you know Spanish?

Leopold: No, but I damn well started to learn, I'll tell you.

Lage: I thought maybe you'd learned that as a youth.

Leopold: No, unfortunately. No, that's a great mistake. Any family that can speak more than one language, if they fail to bring up their children speaking that language, it's a great, great loss. No, I'm very sorry that my mother didn't do that.

Anyhow, this doesn't occur here. In this geology department, for example, a graduate student was once required to have one language, only to be able to read scientific work in that language. Until the day that I left the department, I was the last holdout saying that we must maintain the idea that in order to be a modern scientist you have to read a language other than your own. Practically the day I left this department and retired, they changed it, and now no languages are required.

Lage: I thought it was a requirement of the graduate division.

Leopold: No, ma'am. It's up to the department. A great shame. Anyhow, at Harvard you had to have two languages, and you had to read it.

Lage: Then part of your year at Harvard involved learning these languages.

Leopold: You bet your life. So that there were things that were done in those days that I think were right.

Failure of Modern Science to Pursue the Important Problems

Leopold: I find that modern graduate students in this department really have very little sense of how we got to where we are. The older ideas, many of which posed extremely important problems in our science, are being bypassed by present young people, probably because they never realized how important they were. Let me give you an example.

Geomorphology is the study of landforms, and that means both process and form. What I did, actually, was to more or less help change the nature of geomorphology from a descriptive science into a quantitative science. But we have problems of outstanding importance on which nobody is doing anything. As a matter of fact, after I left the job of chief hydrologist with the Geological Survey and came to California, I could see that I had about ten years of active work left. I made up my mind that I was going to tackle some of the great problems in geomorphology that no one had ever solved.

Now, in science, we have what I think is called the Medawar curve. Dr. Medawar wrote a book on advice to young scientists, and one of the things he said was, there are problems that are so easy that they really aren't worth doing. And there are problems that are so difficult, the chances are ten to one you're not going to solve them anyhow. Therefore most of your effort ought to be spent on problems of the intermediate sort that you think are relatively important but within your scope. I decided the last ten years of my life, I was going to go to the other end of the curve, and I was going to deal with those problems that were so difficult that maybe I couldn't solve them but I was going to make some stab at it.

This is the kind of thinking that we're not getting right now. Let me give an example. I was driving last week between Los Angeles and San Francisco, and going up the San Joaquin Valley and looking at the shape of hills. I had been working on and off on the shape of hills for twenty-five years. The hills that you are passing along Route 5 all have profiles that are convex to the sky, meaning they're shaped like a ball. And then you go into many of the nearby hills, and they're shaped in the opposite direction--they're concave to the sky. Now, geomorphology has to do with the shape of forms. Of the thousands of geomorphologists in the world, I know of nobody right now, except one, I think, who is working on the problem of what determines the shape of hills. That's the science we're supposed to be dealing with, but who's tackling it? It's so obvious. It also is very difficult.

Lage: Obvious and difficult?

Leopold: Yes.

Lage: Is that one you worked on at all?

Leopold: Yes, but I've never published anything on it because my friend Thomas Dunne, University of Washington, and I started fifteen years ago making a collection of surveys of the shape of hills. We have surveys made all over the world now, that we've done--

Lage: You've done the surveys, or you collect other--

Leopold: No, no. He and I have done the surveys together. One of the things that we worked on together was in East Africa, where they have these long hill slopes, oh, a mile and a half long. Very slightly concave to the sky. Tom Dunne has made a tremendous advance in showing by actual measurement how it is that these slopes can develop. As a result, he was elected to the National

Academy of Sciences. It was a very important problem, but it was only one aspect of the problem of the shape of hills.

Now, what I'm saying is that the--. Let me put it this way. I say to my students, "You can waste your life on three small problems." Last year, when I gave a commencement address to the people in the earth sciences here, I reiterated this point. I said, "Don't spend your life on trivia. Pick out problems that are really worth working on." Because as I say, you can waste your life on a few small ones.

Lage: Do you find that your students respond to that?

Leopold: No.

Lage: Even the ones you've worked closely with?

Leopold: No. Fads develop. One aspect of this is that I learned long ago that when a person gets a Ph.D., he or she is going to spend the next year or two years continuing that same subject, regardless of what position he or she is in. When I hired people, particularly when I sent them to school, I just made up my mind, there's no use trying to change their mind. After they get out of the Ph.D., give them a year or two, because they're going to work on it anyhow.

But once that's done, you see, a person has the choice as to what you do next. Unfortunately, the choices being made are continuing to get narrower and narrower and narrower. Instead of sitting back and saying, "All right, now I've finished my Ph.D.; I spent the extra two years after my degree, and I've got as far as I'm going to go with that problem. Now I'll sit back and see what we ought to work on." What I tell students--. Again, I usually tell students to do what I do--

Lage: [laughs] Of course.

Leopold: --and that is, I keep in my file a folder that says, "great ideas," or "big ideas," or "ideas." I said, "Keep a file, a personal private file, in which you write down your thoughts about what are the things that really are most important, whether or not you ever go into them, but keep a file. And once a year, take that file out and read it, and say, 'All right, what did I think in the last year about which were the really important ideas in my field? Am I working on some of these? And if so, what am I contributing?'"

Very few people are taking such a stance, where they're sitting back at times to ask themselves, "Now, what in my science

is worth doing?" I'm not talking about people who are a great genius like Stephen Hawking or people like that, but the ordinary scientist, I fear, is sort of going from one problem to the next one that is kind of an offshoot of the one he did last time.

Lage: Maybe more careerism involved, instead of the larger view of science, do you think?

Leopold: Yes, because--. I think I spoke to you about this before. There's a very grave difficulty now plaguing young scientists, and that is that they think--and a matter of fact, it probably is true--that the way you get ahead, the way you get promoted, and the way you get grants, is to write lots of papers, even if the papers are half a page long. Big problems aren't solved that way, in my opinion. They are not solved that way. You've got to take a job on that lasts a long time, perhaps. But what I say to people is this: Always have more than one string to your bow. Don't work on just one thing. You ought to be doing three or four things simultaneously, and then if one does not pan out, you've got other things you can turn to that are panning out. So that if you do that, you can afford to spend some time on something that is not likely to produce, or that's too difficult.

When I was building a research organization and hiring people, I would say to them, "In choosing something to work on, ask yourself these questions. First, 'Is this something that interests me?' Then, 'Is this something I am capable of doing?' Then, 'Is it possible to do it at all?' because you may pick a problem that there's simply no way to get it done. 'Is there time to do it?' And finally, 'If I do solve the problem that I set up, where does it lead? Can it be expanded by others? Will it be the background for new advances?' So you may turn down a problem because it's either too difficult or you don't have the means to do it; it may require such complicated procedure or money or instruments that you can't do it; or it may be that it requires the kind of skills that you don't have; and finally, it may not be worth working on."

That, I find, is a very unfortunate present difficulty in modern science, in the fields that I know.

Interdisciplinary Resource Planning with the SCS

Lage: Now, to get back into sort of the earlier years, when you were working with the Soil Conservation Service, was there a sense of a mission?

Leopold: Oh, yes. There was no question about that. Oh, yes. In other words, everybody felt a sense of doing something for the land, doing something for the country, preventing the loss of a resource. A real mission. Oh, yes, no question about that.

Lage: So choice of problems maybe was dictated by that in part--

Leopold: But remember, in that kind of an organization, now, you didn't choose problems. We weren't doing research; we were doing an assigned job. But the assigned job allowed us--Tom Maddock and me--to develop new methods, so that in a way it was research, but this was not free research as people do in the university. You were assigned to a group that was doing something.

At that time, there were a lot of new things going on. Now, the group that I was assigned to, it was the first time that anybody in the world had decided that if you're going to do resource planning, it's got to be done in an interdisciplinary way. The team that I was assigned to consisted of an engineer, a soils man, a forester, a hydrologist, and we had at that time a sociologist, but they were sort of not part of the team. Our team consisted of four scientific people.

Lage: And how did that work? That sounds like a really forward-looking idea.

Leopold: Oh, it was. And as a matter of fact, people are now repeating the same thing and getting the same results we got fifty years ago. For example, we were making a series of maps of the watersheds we were working on. A big watershed like the Rio Grande, where you made a map of the rainfall, you made a map of the forest, a map of the soils, a map of runoff, of erosion. This is exactly what is being done now. I saw a group a couple of weeks ago constructing the same kinds of maps we were constructing fifty years ago on the same basin. But I don't think they even knew where to get the material that we had done.

Lage: It would be interesting to compare the results of those.

Leopold: I'll tell you where I saw it. The forest plans that are now being constructed by the Forest Service consist of maps that show the rainfall, the soils, the vegetation, on and on--

Lage: And this is a new thing--interdisciplinary research.

Leopold: Exactly. Actually, some of these plans are redoing what we did fifty years ago, and I don't think they're doing it much better.

Lage: But the land must have changed.

Leopold: Very little. You're not going to change the rainfall. You're not going to change the extent of the forest. You're not going to change the soils. That's really where all land planning begins, you see.

Lage: Did it work well in the Soil Conservation Service? Did this team of people coming from such different approaches do okay together?

Leopold: Very well. Oh, yes, indeed.

Then what we did then was copied by CSIRO in Australia. That was the next group that was doing exactly the same thing.

Lage: Is that a government agency in Australia?

Leopold: Yes, this is the Commonwealth Scientific and Industrial Research Organization, a very important scientific group. I found out some decades after we were doing it that they had also put together teams exactly like ours, again doing very large areas much the way we were doing it.

Lage: And how did the sociologists fit in? You sort of put them off to the side.

Leopold: Because we were at that time, for example, working in the Rio Grande Basin, the question was about the Indian people, and the Spanish-American people. So that one wanted to know, if you're doing land planning, one wanted to know where these people get their livelihood. Where does their water come from? What are the cash crops that they grow? How are the cash crops sold? How do they relate to the matter of credit? How do they relate to the local businesses? Therefore, what kind of planning can be done to maintain and help the indigenous people? So this was a sociological problem.

Land Planning: Need for Responsibility to Society and the Land

Leopold: But of course, land planning in this country has never been done very successfully. Remember these were the days in the New Deal. Planning has taken on rather a bad flavor, despite the need for it. There's such individualism, especially in the commercial enterprises in this country, that planning of a large area is not well accepted. There aren't very many workable tools for enforcing plans. Zoning is the most prevalent one and is a very weak reed, as we all know, because it's so easy to find ways to get variance or to avoid it. The administrative and legislative bodies don't really want to enforce it. So that even the most lucrative kinds of plans for land preservation and development are likely to be turned down by local people.

We worked for some years on a very advanced land plan for a watershed in Pennsylvania called the Brandywine.

Lage: This was with the USGS?

Leopold: Yes, this was when I was in Geological Survey. I was working with people from the University of Pennsylvania and another set of scientific groups, and we were trying to develop a scheme for the preservation of the landscape in this watershed in a very wealthy area near Philadelphia. The idea was that the Ford Foundation was going to put up money to cover the cost of helping people to, in effect, reserve their land following certain land-use practices.

Actually, we had promised to pay them in cash for doing things to show what could be done by proper land planning. For example, don't build on the steepest hillsides. Don't build too close to the streams. Don't build on the floodplains. So basically, we were suggesting that local landowners do not develop their land for the maximum money return, but develop their land in a way so that they get a reasonable return at the same time that they preserve the environment. After a long period of study, the local people turned it down. They are now trying, without our help, to do exactly what we were trying to do twenty years ago. They found, you see, that they are being pushed by the developers who want to be too close to the streams, and they want to build on too steep a hillside.

The whole problem that we've got in this country is this question of the right to do on your own land anything that you want to do without any feeling of responsibility for society as a whole. This is the most regressive idea that any community or society ever had. And this, of course, this was my father's main

idea--that you have a responsibility to other people and to the land itself.

Lage: He seemed to feel that the government couldn't do it, though.

Leopold: Exactly. And that's why we were trying, in our individual way, to work with private landowners and say, "Let us try to help you do the things that we think ought to be done, and try to persuade you that this is in your interest as well as society's."

And of course, you'll hear this idea spoken of again and again as taking property without recompense. The idea is that you must be paid to do anything that you try to do for society, rather than to say, "I have a responsibility to society to do something that goes beyond my own personal interest."

This is the whole game of land and resource problems in the United States. This is the whole question of the ancient timber, the ancient forests, the rainforests, the ozone layer, the carbon dioxide. This is the concatenation of all these resource problems that now we begin to see are affecting everybody. It comes about from the fact that each individual, whether it be a business or a person, may, if he or she wishes, act as if you had no responsibility to the world as a whole.

Lage: That's true. It's sort of built into our whole ethical system.

Leopold: That's right. And that's what my father's essays were all about.

So in the whole question of resources, now--quite apart from the scientific part--the physical scientist has a great part to play, and very few scientists get involved in the relationship of their science to the society or civilization. Somehow or another, the kinds of contributions that are presently needed are contributions that could come from all aspects of the society, including the scientific society.

Lage: But you don't think that many scientists see--

Leopold: No. How many of my students are working on such problems? I can't name any of them.

III WARTIME AND POSTWAR WORK AND STUDIES

Postwar Changes in the Soil Conservation Service##

Lage: On the video you also mentioned some unhappiness when the Soil Conservation Service turned to big engineering solutions. Was that during your time with them?

Leopold: That's a sad story. We at that time, between the Forest Service and the Soil Conservation Service, had developed probably the most active and knowledgeable group of hydrologists in the business. Those two agencies. When the war came along, most of us felt that this work wasn't the most important thing. I resigned from the Soil Conservation Service and joined the Corps of Engineers.

Lage: As a result of war?

Leopold: Yes, I simply said, "I've got to do something else." So I had made contacts during that time with many of the flood control people in the Corps of Engineers, and I wanted to get into work that was more concerned with the war effort. So I resigned from the Soil Conservation Service, and many other people did the same thing, in one form or another. Tom Maddock, for example, went to Central America and became a very important man in growing food in Central America during the wartime. Later he came back, and he and I joined forces again.

But when we came back after the war, we could see that the Soil Conservation Service had turned into an entirely different organization. The chief engineer for the Soil Conservation Service in Washington--I used to remember his name but I don't right now--went to Hugh Bennett, who was the head of the SCS, and essentially convinced Bennett that in order to really get money for soil conservation he was going to have to turn it into an engineering organization, where formerly it had been run by agronomists and geologists and plant ecologists and foresters.

So what they started in on was a program of building dams, both large and small, and we who had this sort of starry-eyed idea of taking care of the land as a whole felt it ought to be done in the most natural way possible, but not by concrete. So none of us went back to the Soil Conservation Service. The service ended up with only one hydrologist out of all those that we had been working with. He was a very good man, but everybody else left, as far as I know.

Lage: This sort of interdisciplinary approach seems to have been lost.

Leopold: Yes, the interdisciplinary approach simply fell apart.

Lage: Would you say that was a casualty of war?

Leopold: No, it was a decision which often is taken by a government agency, that in order to be important they must be big, and in order to be big they had to get money, and in order to get money they had to really change their way of looking at it.

Lage: They had to do things that cost a lot of money.

Leopold: That's correct. So the whole idea of soil conservation was undermined, in my opinion.

Brief Stint with the Army Corps of Engineers

Lage: And what was the Corps of Engineers doing when you worked with them? Were you with them for long?

Leopold: No, for less than a year. The Corps was working on a whole lot of problems having to do primarily with flood control, but mostly with military installations. For example, I was ordered to lay out the desert training camp that General Patton was to be using in the southern Mojave. This was a question of designing a camp for thousands and thousands of people, where you had to deal with water supply, housing, roads, electricity, that sort of thing. Now, most of this actual detail work was done by consulting firms, but engineers within the Corps of Engineers had to make the original design and then supervise the contractors to make the detailed studies.

Lage: Was there anything special that you brought to this, or was the interest just in getting it done quickly? Were you concerned about the effect on the land, that kind of thing?

Leopold: Not under those conditions, no. No, I realized later that--. I didn't realize what a terrible thing the tanks were going to do to the desert.

Enrolling as a Private in the U.S. Army

Lage: You don't think of that during wartime. And then what was your other work during the war?

Leopold: Everybody, of course, wanted to be in uniform. I had advanced up the ladder as a civilian engineer, and I was working under a lieutenant in the Corps of Engineers. I asked him how I could be commissioned as a lieutenant in the army. They found no way of doing it. So I then went to the navy and the Marine Corps. They were looking for engineers, so I was offered a commission in the Marine Corps, and I was offered a commission in the navy. I told my boss I was going to resign my civilian position and take up a military position. I had passed the physical examination, that sort of thing, and decided I was going to go with the navy. They were going to offer me the grade of ensign in the Civil Engineer Corps.

So the day I was to be sworn in, in Los Angeles, I went there early in the morning. I was to be sworn in at eight o'clock. I went to the federal building, and I was about a half an hour early. There was a long flight of marble steps leading up the main door of the federal building, and I was standing on the steps watching, and I saw all these officers walking up the steps. They would get to the door one after another, turn around on their heel and salute. I kept looking, and I thought, "Now, what do they do that for?" I kept looking, and I finally decided they were saluting the flagpole. But there wasn't a flag. I said, "To hell with that."

Lage: [laughs] This is a great story.

Leopold: I turned on my heels and I walked down the street to the nearest recruiting office, and I said to the sergeant, "I want to be a private in the U.S. Army." And he signed me up. [laughter]

Lage: Did people think you were crazy?

Leopold: Oh, yes, of course. So here I was, I was now a private in the army.

Lage: You probably had to do a lot of saluting with that, too.

Meteorological Studies at UCLA

Leopold: Yes, but then I was sent to boot camp and I immediately put in a request for officer training, and I asked to be assigned to meteorological training because it would help my hydrology background. There were three schools of training in meteorology at that time: one at Caltech, one at UCLA, and one at the University of Chicago. My experience had been that anybody who was accepted in the officers' training corps was going to be sent as far as away from where they were as they could be. So I expected to be sent to Chicago. Well, I wasn't. I was sent to UCLA, and here I was, living in Los Angeles already. I was assigned as an officer candidate in the meteorological school at UCLA, and there I spent a year.

Lage: Studying meteorology?

Leopold: Yes.

Lage: Not teaching?

Leopold: No. Oh, no. I didn't know anything about meteorology. But I had a lot of background, you see, from my work at flood control. Practically everybody in our class--there were about thirty-five people--they all came from either physics or mathematics. So I as an engineer was pretty far behind. They all knew a lot more physics than I did. I was also about a half-year older than anybody else; I was twenty-eight.

The end of the first month--the first week, it must have been, I was sure I was going to flunk out. I got a low grade on the examination, and I was so physically being stretched with these terrible calisthenics that we had to do. It wasn't as tough, I think, as the Marine Corps, but we had a very tough program. I was sure I wasn't going to last. Well, it turned out to be one of the great experiences in my life. I finally got the hang of it and I graduated second in the class.

Lage: How did you finally get the hang of it? Did you pick up your math background?

Leopold: I just worked like hell. But also, I had skills that were needed; I was very skillful with anything that had to do with drawing and making maps.

So when we were commissioned, there were about five of us that were asked to stay there and teach as army officers. The next class was much larger than ours, so for the next several years we were teaching meteorology to incoming officer candidates. Our graduates were going all over the world to forecast for the air force. I was in what was called the Air Weather Service.

So finally the war was drawing to a close and we were being reassigned, and I was sent down to one of the air force fields in Texas waiting for assignment. When the assignments came and everybody was being dispersed all over the world, my assignment and my orders read, "Go back to UCLA," because the famous meteorologist who taught at UCLA had his eye on me, and he wanted me to do research for him.

So I went back to UCLA as commanding officer of a small weather station, to do research on low clouds, to work under a professor, Morris Neiberger, who had already been working on the problem of coastal stratus, or coastal clouds. The real idea was that the situation of the coastal clouds in Los Angeles is the exact counterpart of the coastal clouds in Casablanca in Africa, and if we could learn to forecast it in Los Angeles, we could forecast it in Africa. That was what the problem was.

I was now in charge of this little research unit. There was one other officer and about six or eight enlisted men. Every time that I got orders to go overseas, Professor Jacob Bjerknes would phone in General Arnold and say, "No, you can't send Leopold overseas; he has to stay here." So I stayed there the rest of the war and published several papers.

Lage: And you got a master's degree.

Leopold: Yes, and I worked at night to get a master's degree.

Lage: In meteorology, was it?

Leopold: Yes. And I wrote the first paper on the air pollution problem in Los Angeles.

Lage: That was very early.

Leopold: Yes. We were trying to describe exactly what the meteorological situation was as far as air pollution in Los Angeles was concerned. Well, of course, it grew into a great big thing after that.

Lage: I didn't realize it was even very recognized at that point.

Leopold: It wasn't. That's why it was such an advanced idea.

Sedimentation Studies and the Bureau of Reclamation

Leopold: So then when we were mustered out of the Army, then I had to decide what I was going to do.

Lage: Were you married by this time?

Leopold: Yes. At that time, a person was discharged at the camp nearest his main residence, and my main residence was Wisconsin. So I was discharged at Fort Douglas in Wisconsin. In the meantime, I was in conversation with people that I had known, one of whom was in the Bureau of Reclamation, and he offered me a job. So I went to Washington as a civil engineer in the Bureau of Reclamation.

The man that hired me was sort of a, not a distant relative, but he was connected by marriage with someone in my family, and he offered me this job. I got there and I said, "Now, I want you to know that I'm really not a believer in what the Bureau of Reclamation does. If I take this job, I want you to know that, because I've been in flood control now for a long time, and I don't believe you're going the way you ought to go. But I think I can contribute something." "That's all right."

So two things transpired: one, the main hydrologic work in the Bureau of Reclamation was going on in Denver, and as part of this work that I was doing I was assigned to go to Denver to assist in some problems that were coming up in the Rio Grande. But the Rio Grande is what I had studied in the Soil Conservation Service. So I went there with the chief hydrologist, Randy Riter, and we went to a meeting, and I had made a study of recent data on the Rio Grande. He was so impressed with what I had done that he said, "Why don't you come to Denver and be in my department?" I said, "No, I don't think I want to do that."

But I said, "I'll tell you what I think you need." I said to the people in Washington, "You don't know anything about sediment. You'd better know something about sediment, because you're going to have a lot of problems with it, and I suggest that we set up a sedimentation section." Well, I sold it and I set up a sedimentation section, built a big laboratory, and got the Bureau of Reclamation interested in sediment which, of course, I've followed up on the rest of my career.

Lage: Was that something the Bureau just hadn't paid much mind to in the past? It seems awfully important for their work.

Leopold: You'd think that they'd have realized it, but for some reason they didn't. But when the chief of the hydrology section got involved in the Rio Grande question and I gave some assistance to him in understanding the sediment problem, he began to realize that sediment was important to him, and therefore he got behind the idea that I had proposed and the formation of a section on sedimentation.

And then I brought my friend Tom Maddock in. At the end of the war he was looking for a job, and I persuaded the Bureau of Reclamation to take him on. I think that's the sequence. He joined the Bureau of Reclamation at my suggestion and was very important in getting their sediment business started. Shortly after that, I left the bureau and went to Hawaii. Tom Maddock was with the Bureau for many years and was of great assistance to them because he was a very practical engineer with a lot of knowledge about western conditions.

Lage: You had mentioned in the video a sedimentation survey of Lake Mead. Is that something you were in on or that you just got going?

Leopold: Well, I was certainly in on it, but I wasn't really responsible for it. I was much concerned with it at the time, yes. But I was simply a collateral player in that game.

Lage: From what I've heard of the bureau's role in the water controversies in the Southwest, it seems as if they haven't taken account of the problems of sedimentation. How does the research end up in the project planning?

Leopold: Well, you see, what happened was that they got a group of very good people when the sedimentation section was first started. When those people retired, the whole section went to pot. As far as I can see now, no sediment work is being done that I know of. As a matter of fact, the laboratory that I had them construct has really never been used for the purposes I had in mind.

Lage: That's discouraging.

You also mentioned cooperation between the bureau and the navy and the Geological Survey during this time back in '46.

Leopold: Yes. When it was decided among the many of us that there was to be a sedimentation survey of Lake Mead, it's a big lake, and therefore we needed essentially naval vessels. So that Mr.

Cummings from the navy was a scientist who was of great assistance in getting equipment that the navy could produce onto the Lake Mead survey. So the combination was the navy produced primarily the equipment, the bureau mapped the reservoir, and the Geological Survey put the man in charge who was the technical supervisor of the job.

Lage: What was the Bureau of Reclamation like to work for? How would you characterize it at that time?

Leopold: Well, I guess I told you that when I went to work for them, I said, "I really don't believe in what you're doing. I'm going to try to assist, but I'm not a believer in big dams." But after all, that was just one person making a statement.

I don't know how to characterize it; I was there such a short time. It tended to be quite bureaucratic and obviously very political. The people that I got to know later on in the secretary's office were, in my opinion, a much broader kind of people. At that time, you see, there was a tremendous push for the combination of flood control, irrigation, and power. That was what was driving the Interior Department. Later, when the administration changed and the situation began to change, especially when Kennedy put Stewart Udall in as secretary of the interior, then there was an entirely different point of view.

Lage: And in the public as well.

Leopold: Yes. A gradual change in the public view too.

Lage: Do you think you would have stayed on there long if the Hawaii opportunity hadn't come up? Were you dissatisfied? Is that part of the reason for the change?

Leopold: Oh, no, I wouldn't have stayed in the Bureau of Reclamation. I was looking for other things to do.

Lage: Was your interest in sediment something that came out of your experience with the Soil Conservation Service?

Leopold: It's a whole business of how meteorology fits with hydrology, fits with sediments, fits with floods, and the whole thing about water development. It was another step forward.

Lage: So meteorology focused into this.

Leopold: Oh, meteorology was very important to me because it made me think of things in a different way than other people thought about them.

Lage: Was the Harvard professor right, that it's climate that makes the difference?

Leopold: Well, then let me tell you about that. Yes, but nobody had proven it, you see. The Soil Conservation Service said, "Man and overgrazing has wrecked the whole world," and the Harvard professor said, "No, you haven't thought about changes in climate."

Meteorologist for the Pineapple Research Institute in Hawaii.
1946-1949

Leopold: So working at the Bureau of Reclamation in Washington--I had been there little less than a year--I had a telephone call from probably the most important meteorologist-hydrologist in the country, Merle Bernard, who worked for the Weather Bureau. He said, "There's a man in town from Hawaii who's looking for a meteorologist, and he'd like to meet you. Would you like to talk with him?" I said, "Yes, I'll talk to him."

So I went to meet this gentleman who had been formerly a very important man in the research unit in the Department of Agriculture, and he said, "I would like to have you meet me at the Mayflower Hotel for breakfast." I went to breakfast with him, and we talked, and he said, "I would like to have a meteorologist come to Hawaii to be the head meteorologist, to develop a scheme of forecasting for us. We are particularly interested in long-range forecasts for both pineapple and sugar, because," he said, "the organization I head, called the Pineapple Research Institute, is supported by the sugar people and the pineapple people."

Well, during the discussion I said, "Dr. Achter, I think that you can get a lot of help out of short-term forecasts, but I've been in meteorology enough to tell you point-blank that you're not going to get any long-range forecasts. We are not able to forecast more than two days ahead, and if over a period of a decade we can forecast three or four days ahead, we will be doing very well." But I said, "I will not be hired with the expectation that I'm going to develop long-range forecasting for you. But I can tell you that meteorology is something that will help you."

So further discussion, another breakfast, and then I called my father, and I said, "I have this opportunity to go to Hawaii,

to be a meteorologist. What would you think?" Dad never gave any advice to anybody. He said, "I'm very glad that you have this opportunity. It's something that you ought to consider very carefully," but he refused to commit himself. He was not going to try to influence one way or another.

Lage: Would he give you suggestions to think about?

Leopold: I don't remember--. Yes, of course, but I don't remember what they were. But he would not help decide. He would only make you think about them.

So the last meeting with Dr. Achter I said, "Sir, I'll go to Hawaii, but I can't do it unless you double my present salary." "Oh," he said, "that's no problem." Well, then I was stuck.

Lage: Then you had to do it. [laughs]

Leopold: So now I went to a very--in those days, a very highly paid job, and--

Lage: Who were you actually working for?

Leopold: The Pineapple Research Institute.

Lage: Not the Weather Bureau.

Leopold: No. No, then I was in competition with the Weather Bureau, you see. Because now I'm the foremost meteorologist in Hawaii, and start showing up the Weather Bureau, who weren't doing what they were supposed to do.

Lage: But was it in cooperation with the Weather Bureau?

Leopold: I tried to develop cooperation with the Weather Bureau, but they felt I was intruding on their business. And indeed, they didn't have any new, young ideas, you see.

Another Lesson in Supervisory Styles

Leopold: Well, anyhow, I got to Hawaii. They paid my expenses, and it was wonderful. I got there, and of course, I'd never been to Hawaii before. It was just a marvelous experience. Beautiful climate, and everything was lovely. They furnished me with an office, a beautiful secretary, a car, and a big salary. I waited for the director to tell me something. I tried to find out something,

but nothing happened, nothing happened, nothing happened. Three weeks. Finally I had a call from the director's office. He wanted to talk to me. They had taken me everywhere and wined and dined and everything.

So I went in his office, and he said, "I suppose by this time you would like to know what we want you to do." I said, "Dr. Achter, I'm so glad to talk to you. It's exactly what I've been waiting for. I want to know what you want me to do." He said, "Well, I want to tell you what I want you to do. I want you to do nothing." I said, "Nothing?" He said, "Yes." He said, "I want you to travel, I want you to get to know everybody on the island, all the islands." He said, "You're the only one that can freely travel to all the islands, because some of the islands have only sugar, some have only pineapple, but you work for both. Go everywhere. Meet all the plantation people, learn all you can about pineapple and about sugar, and don't do anything for a couple of years. Just learn."

Lage: That's an interesting assignment.

Leopold: Well, you see, now I'm beginning more and more to find out what it is to be a supervisor.

Lage: Right.

Leopold: So that's exactly what I did.

Lage: How did you react to that?

Leopold: Wonderful. I was a free agent.

A month went by, and I was called into the director's office. He said, "Luna, you haven't followed my instructions." I said, "How so, sir?" He said, "I have here on my desk a manuscript that you've written, that you want my permission to publish. But," he said, "I told you to do nothing. And here you've been doing research, and you've already written a manuscript." He said, "You haven't followed what I told you to do." I said, "Yes, that's correct." "Well," he said, "you know? I have had several department heads who have never written a paper in the last ten years." Of course, he was pleased as punch, but that was the way he greeted me. So I--

Lage: Was this a big outfit, this Pineapple Research Institute?

Leopold: Yes, indeed. It was not very large in numbers, but there were very important people in charge of different departments. I was the chief of meteorology. There was a plant physiology section,

a mechanical engineering section where they designed equipment for harvesting. There was a soils section. There was a plant genetics section. There was an entomology section. And mine was the newest of all of the sections.

Lage: And was it funded by the sugar--

Leopold: By the sugar and pineapple people.

Lage: It sounds like sort of an agricultural experiment station.

Leopold: It was an agricultural experiment station.

Lage: But not connected with the university?

Leopold: We were on university property, but we--. We took over some university buildings and then built an absolutely beautiful research building. Later on, after I left, hard times fell on both sugar and pineapple and they discontinued it. But in the meantime, a lot of interesting things happened. It was a wonderful experience.

Rainfall Maps and Records

Leopold: For example, I was constructing rainfall maps, you see, the same thing that I had been doing before, and now being a meteorologist I knew a lot more about things of this kind. An argument grew up between one of the sugar companies on the island of Maui and the territorial government. The territorial government, interestingly, was being represented by the United States Geological Survey. The sugar company was paying the territory for water which fell on territorial lands, which were drawn from a ditch coming along the mountainside into the sugar and pineapple plantations, sugar primarily. The payment to the territory--the territory, you see, was part of the United States government now; Hawaii was not yet a state--was based on the rainfall map.

It was decided by both agencies that the rainfall map was probably wrong. They wanted somebody to make an independent study which would not be influenced either by the agriculture people or the federal government. They came and asked me if I would do it. I said, "I'm very busy doing what I'm doing, but I'll tell you what I will do for you. I will lay out the methods by which this could be done, and then you could have it carried out by somebody else."

Well, I got so interested in it that I did it myself and wrote a paper that was published as "The Rainfall of East Maui." It was a study of the rain-gauge records, you see. Then I had to go back into the geological record, particularly pollen, to see what the rainfall record had probably been in the Holocene, in the last ten thousand years. As it turned out, in the central part of east Maui I raised the annual rainfall by 150 inches.

Lage: You found 150 more inches?

Leopold: Yes. So it made a lot of difference about who paid what. Anyhow, it was a very interesting assignment. But it was a kind of a sidelight, you see.

Lage: Did it result in the plantation owners having to pay more?

Leopold: I don't even remember, because that was not my problem. My problem was to make a new map, which I did.

But immediately when I got to Hawaii, I began to realize that rainfall was everything, and therefore I had to know not only about irrigation, but I had to know a lot about rainfall. So I started to make a study of the rainfall records in Hawaii. There were published, I think, six gauges. When I got through with my study, I found 650 gauges.

Lage: You found them already there?

Leopold: They were there. Nobody knew about them, because each plantation, you see, was doing certain things, and I brought them all together and made a rainfall map of everything with all the gauges shown and where they were and how long they had been there.

Lage: And was the data accurate at these various gauges?

Leopold: But then that was a question I had to deal with, you see. I had to now deal with the accuracy of the data, so I had to make a study of what gauges could be trusted and what gauges couldn't. There was a lot of interesting stuff.

Lage: A lot of traveling around and really getting to know the--

Leopold: I loved it. Oh, yes, I loved it. I had an airplane and I had a jeep on each of two islands, and it was great.

Developing a New Rain Forecasting Scheme in Hawaii##

Lage: Had your work in Hawaii come to a turning point?

Leopold: No, no. I had developed a new forecasting scheme that was already in place; I was forecasting in a very new way, a lot of new ideas.

Lage: Short-term forecasting?

Leopold: Yes. I developed a scheme which nobody had ever done before. My scheme allowed me to make a forecast of the rainfall, field by field, all over the whole island of Oahu. I had worked up cooperative relationships with universities on the mainland and was getting help from a lot of scientists on the mainland; that's another whole story. But we had a forecasting scheme in operation.

Lage: Did this affect the pineapple and sugar people's decision making?

Leopold: Yes. I really couldn't forecast the small rains very much, but I did pretty well on the large rains. I went to Washington and I talked to the chief of the Weather Bureau in Washington, and I said, "I now have a scheme which I would like to put on the radio. My scheme involves the following things. I want to forecast the rainfall in amounts. In other words, I'm going to tell you how many inches are going to fall twenty-four hours in advance. I want to put it on the radio, but I'm going to put, also, a probability forecast. I'm going to say this is a 75 percent chance or a 90 percent chance, to tell people how sure I am." And he said, "Oh, that's much too advanced. You can't do that." Of course, that's what's done every day now.

Lage: Right. But it's many years after you came up with that idea.

Leopold: Yes. Many years. Anyhow, they said that.

So I went back to Hawaii and I said, "Now, what I'm going to do is when I see something that's important, I'm going to start phoning the pineapple companies and sugar companies and tell them, 'Look, two days from now you're going to get such and such. And it's going to fall on these fields, and this is how much it's going to be.'" Shortly after that there was a big storm coming in and I phoned the main people on the islands, told them what I thought was going to happen, and everybody then stopped burning cane, took their machines off the fields--cost them many, many dollars--except one. The Ewa Plantation said, "To hell with that. We're going to do what we're going to do." The rain came

exactly as I forecast, and they lost about a quarter of a million dollars. Which paid for my operation in full. And then they began to pay attention.

Lage: Now, was it the equipment that the rain would ruin?

Leopold: Yes, because you see, you burn ten acres of cane, or five acres of cane. Now, it's lying on the ground. You have to get it to the mill before it decomposes, and you have to get it there with heavy equipment. But the heavy equipment was stuck in the mud. So you had both the canes on the ground and the heavy equipment can't move because of the mud.

Lage: So it really was important.

Leopold: So they had a big loss, and all of a sudden they began to pay attention to the fact that I was furnishing them with a service that was important.

Experiments with Cloud Seeding

Lage: I noticed in your journal on the Hawaiian years something that looked intriguing. You can tell me if it was or not. The seeding of clouds.

Leopold: Well, at that time, the first scientific papers had come out on this. In the eastern United States, they started out with laboratory experiments, but then one began to see that under certain conditions, if you could supercool the cloud droplets, that you're going to cause rain. Well, since that was one of the main things that the pineapple and the sugar people were interested in, I decided I was going to try it.

Lage: They needed more rain? Or just rain when they wanted it?

Leopold: Well, they needed more rain, in the summertime especially. And you see, on the dry parts of these islands--you have a lot of rain on the windward side, but the dry sides are very dry.

So with the permission of both the sugar companies and the pineapple companies and my boss, we started to try it. This was very early in the game when not much was known about it. We had lapse-time photographs of how the clouds built up when we seeded it with dry ice. This we carried on for some months. On one day, such a tremendous rain occurred on the island of Lanai we practically washed them out.

Lage: I saw a picture in your journal of the real floodlike situation.

Leopold: Some of the companies got really very interested because it looked like it might work. At the same time, Dr. Langmuir from Schenectady was trying to determine why it should work under those circumstances, because in the physical theory, there was no reason why it should work under these tropical conditions. Langmuir wrote a paper in which he used our data to try to give an explanation for what was happening.

Well, this had gone on for, I suppose, close to a year. I said, "I'm dissatisfied with this business because we're just testing now. We have to have an experiment that's properly designed." I designed an experiment in which we were to draw by random lot when the seeding was to be done and where it was to be done. We had a list of places that seeding might be done and under what circumstances. There's no use seeding when there's no possibility--when there are no clouds, for example. Therefore, once the conditions were right, then by drawing lots, I had recommended that we were going to seed at the place that the card showed. And the draw of the card would determine also whether to seed or not to seed. In other words, was the rain going to fall in the absence of seeding?

I presented this to the companies. The Libby Company had a chief scientist who said, "Look, you work for us. We pay you. If you say that the conditions are right for seeding, we're going to seed regardless of what you do." "Well," I said, "that would ruin the experiment." "We don't care. That's what we're going to do." I said, "Very well, I cancel everything. I will not do any more seeding." And I never did. From that day on I never touched it again, because if they wouldn't allow me to do a proper experiment, I wasn't going to continue to have anything to do with it. So the thing just fell apart.

Lage: I see. Was there any public response to this? If you did it today, I--

Leopold: Oh, a tremendous public interest, but as knowledge grew, we could see that in the long run it was not trustworthy at all. My published papers show that. I couldn't prove that the rain that occurred was really due to our seeding. And that was the reason I wanted a scientifically designed experiment. If they didn't want to run an experiment, I said I didn't want to do it at all, so I just quit.

Lage: They didn't try to get somebody else?

Leopold: Shortly thereafter, I left Hawaii, and the man who took my place, who was my colleague at the time, he tried to continue it. In order to do so, he brought in some very high-powered talent from universities in various parts of the country. It simply frittered away. I was away from it and therefore I didn't know, really, what happened. But it didn't come to anything.

Lage: It hasn't come to anything else, now, has it?

Leopold: No. In other words, the more people got into it--and there were lots of people who really put a lot of effort into it--it simply is not dependable and you can put it in one sentence. At the time you need the rain, the clouds are not in a favorable situation. In other words, when you need the rain the most, it's not possible to get any inducement.

Lage: You have to have the clouds to begin with.

Leopold: So in effect, the times that you need it most to make it is the most impossible time to get any effect.

Lage: Your journal talks about a trip you made to Washington in the middle of this assignment.

Leopold: Oh, many trips to Washington. Many trips, yes.

Lage: It sounds as if you had relationships with the Weather Bureau that had to be worked out.

Leopold: Indeed. As a matter of fact, I worked very closely with the chief of the Weather Bureau, because I was furnishing a forecasting system that they should have been furnishing, you see. And I was trying to bring them into a cooperative agreement with me, so we could do these things jointly. They were very slow to respond. They did finally begin to respond, but it was kind of touch and go because, you see, they felt I was a competitor. Or we were competitors.

Four Months to a Ph.D. in Geology at Harvard. 1950

Leopold: In the meantime, I went back to some of the things that I had been doing when I was in the Soil Conservation Service before the war. I was completing a paper that I had started in the Bureau of Reclamation on the history of what the early explorers had found when they first went to the West in the late nineteenth century. I was writing a paper on what I was calling the

vegetation in the Southwest in the nineteenth century. You see, now I'm talking about biology.

Leopold: So I finished this manuscript. I had been in correspondence over a good many years, since 1937--this was now 1950--with Professor Bryan at Harvard. He had earlier said, "Yes, you can come back to Harvard and finish your degree if you take this, that, that," --things that didn't interest me and that I simply wasn't trained to do. But I sent him this manuscript, and he wrote back a letter that changed my life. It was only two sentences. It said, "Why don't you come back to Harvard and use this manuscript as the beginning of your doctor's thesis?"

So I said, "I'm going to go." I had been earning a large salary so I had put my money aside, and I figured, "If I spend all my savings of five years, I'll take my family to Harvard," which is a lot of expense. But then when I went to my director, he said, "I'll tell you. We will pay for your schooling."

Lage: The Pineapple Research Institute?

Leopold: Yes. I said, "All right. I would like to have you tell me, though, what would be my responsibilities? Because I can't accept this without knowing what's expected of me when I come back." They put off and put off, and they wouldn't really specify. So I said, "No, I can't do that." I said, "I cannot tie myself down to something when I don't know what I'm expected to do. I would rather go on my own, and then if you want me to come back, that's another matter."

So here I am; I packed up my family and I went to Cambridge, used up all my savings. Got there on the second of January, and in the next three weeks I took two language examinations, I passed my orals, and then I took four courses, wrote my thesis, and left with a Ph.D. in four months. No one had ever done this at Harvard before.

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Lage: Tell me more about Harvard and the Ph.D. studies. Weren't you working for the Geological Survey when you went to Harvard?

Leopold: Yes. The Geological Survey hired me when I left Hawaii, and I worked for them for a couple of months.

Lage: In Los Angeles.

Leopold: In Los Angeles, yes. That's where I was stationed. Then I took leave without pay, went to Harvard for half a year, and then came

to Washington after that. So that yes, I was employed but I wasn't being paid. In other words, I was on leave.

Lage: Was that all set up before you came to the Geological Survey?

Leopold: No. Well, yes, in a way it was, although I think the survey people had not the slightest idea what I was going to do, because they had never hired a research man before.

Lage: Tell me about that. How did they hire you as a research man, and why?

Leopold: Well, I told you that while I was with the Bureau of Reclamation, I had made a good impression on the chief hydraulic engineer of the USGS. About the time I wanted to go to Harvard, the chief of one of the branches came to visit his offices in Hawaii. I had him for dinner, and I said to him, "I'd like to remind you that five years ago, the chief said that if I wanted to come back to Washington and the Geological Survey, that they would give me consideration." I said, "I wonder if you would be good enough to take that message to the chief saying, 'Yes, I would like to do that.'"

He said thank you, he would do that, so that the arrangements were made then that they knew I was going to come back anyhow to go to Harvard, so they said very well--I was paying my own way--I could report for work in Los Angeles and take leave and then be reassigned.

Lage: Was the work in Los Angeles research also?

Leopold: No. I worked for them about two months. I came to Los Angeles, and they didn't know what to do with me; they didn't have any idea what I was hired for. But nobody did. It was my friend Walter Langbein who had persuaded the chief hydraulic engineer that I'd be a good person to have around. But nobody knew what I was supposed to do. So I got to Los Angeles and they said, "Well, there's a desk, but you'll have to make up your mind because we don't do that kind of work that you expect to do."

Lage: You were the first research person in the division?

Leopold: Yes. They'd never had one before. So I said, "Fine." So without even sitting down at my desk, I said, "I'm going to New Mexico." I had been now away for five years. So I went to New Mexico and picked up with some of my old colleagues there. When I got there, I was working on one paper for my thesis.

Lage: The Southwest vegetation?

Leopold: No, it was the one on the climate of the Pleistocene. I was working on evaporation.

Because I wanted to see my USGS friends in New Mexico, I'd gone to New Mexico. I was supposed to meet one of their administrators, Mr. Peterson, at the hotel in Gallup at a certain time. I went to the hotel after the train arrived and he wasn't there, so I just put my baggage behind the desk and I put on my boots and I walked out of the hotel down to the river, which was right past the hotel. Within two hours I had discovered things that I had never seen before and nobody had ever seen before. I started mapping the geology that I saw.

I came back a couple of hours later and met Mr. Peterson. I said, "You ought to see what I found. It's very, very interesting. I found some ancient material that is exposed in the gully." So we went out in the field to see some of the things that he was doing. Every time that I saw something interesting, I'd have the car stopped and I'd rush over and take a look and make a sketch. They didn't know what the hell I was doing. So at the end of two days, I had a paper ready to write because I had discovered a whole lot of things that no one had ever seen before, which was right along the line of my major professor at Harvard that I was going to work under.

Lage: Was this again looking at the past?

Leopold: Yes, this was looking at the geologic section and seeing what the climate had been.

So at the end of this two or three days, I went back to Los Angeles and sat down and wrote a paper, which then became part of my doctor's thesis.

And then when I was in Los Angeles, I was living in west Los Angeles and my office was in the federal building in the center of Los Angeles, so I had to take the bus. The bus trip took three-quarters of an hour, so I studied French twice a day on the bus. By the time I got to Harvard I could pass my examination in French. [laughter]

Lage: You really make good use of your time, I must say.

Leopold: I was young. [laughs]

Lage: So that's one reason you got through Harvard so quickly. Or got the requirements finished.

Leopold: Yes.

Lage: In that short time at Harvard were there any important experiences, or did your major professor have a particular impact on you?

Leopold: Oh, yes, indeed. But this was, you see, the second time I had worked under him. In other words, I worked under him in 1937 and this was 1950, and now I came back, you see, much more senior; I had written quite a few papers. He took a tremendous satisfaction from my being there, because now I was older than most of these other people that he had had.

I owe everything to him because he simply said to me one time, "The day you decided to leave Hawaii, you earned your degree." He said, "The fact that you were willing to spend your own money to come back here and study and get your degree, that means that I'm going to--." In effect, he was saying, "I'm going to see to it you get your degree in three months," which he did.

He was happy as the Dickens because he had--. Compared with the amount of time that we spend on graduate students here-- helping them and reading their manuscripts and giving them ideas and all that sort of thing--that's not the way Dr. Bryan worked at all. The greatest help that he gave me was one letter consisting of two sentences. That's the supervision I had. One letter, two sentences. And it said, "I wish that you would consider the problem of what was the climate in the Pleistocene in the Ice Age." That was it. I said, "Well, if that's what he wants to do, that's what I'm going to do." The paper I wrote became very famous, and he was very pleased with it. Extremely pleased. Because I was attacking it from a way that no one had ever thought about before, looking at it as a meteorologist as well as a geologist. Bryan was very pleased with that.

Lage: Was it your meteorological training that provided the new input?

Leopold: Well, in this respect. I went from the published change in the height of the snowline, which had been published by geologists, and made the meteorological assumption, which later turned out to be a reasonable statement, that the so-called lapse rate--in other words, the rate of change of temperature--remained the same in the Pleistocene as the present. That was a meteorological assumption that was very important. It turned out that everybody agreed. When they saw it, they said, "Yes, that's the way it ought to be."

One of the people on my committee wrote a letter to another member of my committee at Harvard. This was the great Russian

climatologist whose name was Konrad. Reading this paper that I had written, he wrote a letter to the other professor, which I of course was not supposed to see, but after I got my degree Dr. Brooks gave me the letter. Konrad said, "It is not right for a young man to work on a problem so complicated as the climate of the Pleistocene. That should be left for the end of his career." [laughter]

Lage: That was his objection?

Leopold: Yes. And Bryan and Brooks laughed to themselves. They simply didn't pay any attention, but he objected strongly to my working on a problem that was a problem of speculation. [laughs]

Lage: And your Ph.D. was in geology?

Leopold: Yes.

IV THE LEOPOLD FAMILY, THE SHACK, AND A SAND COUNTY ALMANACCompetitive Relationship with Starker##

- Lage: After last session, I realized that, in an effort not to repeat too much of all the written material, we really haven't talked enough about your family, particularly your recollections of your brothers and sisters, and growing up in Wisconsin. Do you want to talk a little bit about your brothers and sisters?
- Leopold: Well, of course, Starker and I were two years apart in age, but we were much closer together in schooling. We were only one year apart in schooling, so that the early part of our childhood was very competitive. Extremely competitive.
- Lage: Did you both feel that, do you think, or you as the younger brother?
- Leopold: I don't think I posed much of a problem for him, but we were competitive in a lot of ways, girls among other things. And then there was a long period of time in which we practically didn't speak to each other.
- Lage: What age was that?
- Leopold: That must have been from, oh, I just don't remember. I know there was a long period when we didn't seem very--
- Lage: When you were an adult, or still a--
- Leopold: I think it must have been late high school, early college. But my brother Starker was a very popular man who in high school was on the hockey team. I never went out for any team, and I was very shy and sort of retarded in high school. Never had a--
- Lage: I'm sure that's not the right word. [laughs]

Leopold: Well, I had no confidence in myself. When Starker started college he didn't work very hard, and he joined a fraternity and ended up by flunking out. Well, to make up for that, I made up my mind--and of course my family was very upset about this--I was going to be the guy that got good grades and showed him up, at which I worked very, very hard, and was kind of the opposite of him.

Lage: I see. And consciously sort of--

Leopold: Oh, yes indeed. But then, of course, after Starker went to work after he flunked out of college, he then came back and made an extremely good record for himself. So it didn't last. But in the meantime, I took it on myself, as I say, to be in an entirely different camp. I wouldn't join a fraternity, for example, and I got very good grades. The turning point in my life was at the end of my freshman year of college when I was just fifteen. I made the crew of the first boat, and that changed my life because all of a sudden now I found that I, also, could be successful in sports. And then immediately after that, then, I was soon elected to the honor societies, and that really was a very, very great change in my life, that all of a sudden I became successful.

Lage: The hard work paid off.

Leopold: Yes.

Lage: Now, do you think Starker at that point began to feel more competitive from his end?

Leopold: No. Starker, you see, had gone off. After that we didn't see each other for many, many years. He went off to get his Ph.D. [at University of California, Berkeley] so that we weren't thrown together very much. He took his bachelor's at Wisconsin, Master's in forestry at Yale, and then Berkeley. This was in the period 1939-41 approximately. During the war he was doing research in Mexico.

Lage: But you did go on hunting trips together, it seems from your journals, as a youth and teenager.

Leopold: At that time, yes, we did quite a few things together, although there was still a lot of competition because, for example, he started dating the girl I was in love with.

Lage: How about your parents' reactions to the two of you? How did they deal with this one very self-disciplined youth and one not so disciplined?

Leopold: They were very understanding of both the personalities. Very understanding. No comment was ever made that made Starker feel bad for not doing very well in school, and very little praise was heaped on me, but my father would say--. Just before they went to bed I'd have been studying for three or four hours, and my father would say, "I think you study too much. Why don't you study a little less? This is not worthwhile." They were very understanding; never any criticism about either of the boys.

Lage: That's hard to do as a parent, to remove yourself and not be critical.

Leopold: Yes, but they were extremely unusual parents. For example, right about the time that Starker was about ready to flunk out, he was going with a sorority girl. He went off to have a party with some boys and asked his girl to drive our family car back to town. The car was brand new, and buying a car in those days, particularly when my father earned so little money, was a very important matter. On the way back she wrecked the car, and I mean wrecked it completely. I remember my father came in the house, and my mother said to him, "What's-her-name has just completely ruined our car." His reply was, "I hope she wasn't hurt." "No," my mother said, "she wasn't hurt." My father said, "Stella, I think you'd better invite her to dinner tomorrow night so that she doesn't think we're angry at her." This was quite a blow, and a very great financial blow, but that was the kind of reaction he had.

Financial Hardships in the Depression Years

Lage: Did the tightness of the financial situation affect you?

Leopold: Oh, yes, indeed. I don't know how my mother made out with five children on the amount of money that she was given. She never knew anything about the family finances. She took what my father gave her, and that was that. And she made out somehow or another. We were never bothered about it; we were never told much--nothing in detail.

Lage: It wasn't discussed as a family problem.

Leopold: No. I saw once in a while my mother in tears, but it was not imposed upon the children, nor did you have to say anything, because everybody knew that there wasn't very much money.

Lage: You didn't ask to go away to college, for instance. Or did you? Was that ever a thought?

Leopold: No, we were perfectly happy to go to Wisconsin. A very good school indeed, that of course wasn't costing very much in those days. Sixty-four dollars a semester.

Lage: And living at home?

Leopold: Yes, and living at home. But for example, we never took the bus to school. The nearest edge of the campus was just a mile from our house, and we walked four times a day. We walked to school, walked back for lunch, walked to school in the afternoon, and walked home, regardless of weather. Well, friends of ours would take the bus, but we apparently didn't have ten cents to take the bus.

Lage: Was this when your father wasn't working?

Leopold: Oh, yes. When the Depression came, he was working for an arms and ammunition company. He had just quit his job at the Forest Service, and he was making this famous survey--the game survey, first of Iowa and then of the midwestern states. When the Depression caused that job to disappear and he was really without work, he just sat down and wrote a book. Then, as a result of a series of lectures that he gave at the time that this book was being written, arrangements were made by the dean of the university to create a professorship for him. But this was a very trying time for my mother and father because they were really short of money.

Lage: As you grew older and had your own family, did you see this period of financial hardship as being a molding experience?

Leopold: Yes, because I think that most young people go through something like this. Most of the people that I know started out pretty much from scratch and didn't get very much help from their families. We certainly didn't get any help from our family, but nobody talked much about it; you were expected to go off and do it, that's all. But yes, it was like any other important experience: you learn something about how to do it properly, not because you discuss it, because you just thought about how it was done.

Carl, Nina, and Estella Leopold

[Interview 3: June 6, 1990]##

Lage: Could we talk a little bit about your younger brother and sisters?

Leopold: They're all very distinguished people.

My younger brother, Carl, took his Ph.D. at Harvard, and he is distinguished professor of plant physiology at Cornell. Actually, he sort of divides his time between the Boyce Thompson Institute and the University at Cornell. He, I understand, just recently retired but keeps up his research. He's very much interested in training of scientists, the whole matter of ethics in science, as I am. His wife, interestingly, runs the largest recycling environmental group, apparently, in the state.

My sister Nina is married to a geologist. They live on the Leopold Memorial Reserve at Baraboo, Wisconsin, and she's made a big reputation for herself in the ability to restore original prairies with original plants. A year ago she and her husband were each given an honorary doctor's degree from the University of Wisconsin, which is quite unusual, to have a couple, each honored by a big university. They lead essentially a kind of research life. They have a great garden. I never saw anything like it. It's a garden that's big enough to feed a whole town, I think. So they're great plant horticulturists.

My sister Estella took her Ph.D. at Yale, and she worked for quite a while in the United States Geological Survey as a palynologist [student of fossil pollen]; she's a pollen expert and now is professor of botany at the University of Washington, Seattle, and a very distinguished scientist. She's a member of the National Academy and is a great conservationist. She and her friend, Vim Wright, were responsible with a few other people for saving the great fossil beds called the Florrisant in Colorado. And they actually did sit down in front of the bulldozers. They were trying to get this very famous paleontology site, which has insects and leaves in it, declared a national monument.

Lage: What were the bulldozers proposing to do?

Leopold: The bulldozers were going to build houses on the fossil bed. They got the bill through Congress, and the thing is protected now.

Lage: So the whole family combines the scientific interest and the ethical-ecological interest.

Leopold: No question about it, yes.

Lage: As the girls were growing up, were there the same expectations for them as for the three boys, in terms of schooling, for instance? Did you see a difference in treatment?

Leopold: My younger sister Estella, of course, is eleven years younger than I, so that she was home for a much longer time and without any siblings at home. So basically I think she probably knew my father and mother more than the rest of us did when we grew up, because everybody was so close together that--.

There's a tremendous difference between my older brother and I, although we're two years apart, on the one hand, and my younger sister and younger brother, on the other. They were also two years younger than ourselves, but it was as if there were ten years difference.

Lage: Why is that, do you think?

Leopold: I don't know. I really don't know. Well, it was partly because Starker and I went to one high school, and then when the other children got into high school, they went to another high school, so they had another group of friends, and they always seemed much younger than we were. It's hard to explain. I don't understand it, but it's as if the difference in age were much larger than it really was.

Lage: Did you do things with Carl like the hunting and fishing trips?

Leopold: That's the point. Not so much.

Lage: More with Starker.

Leopold: Later, after we were all out of college, then it began to change. But when we were growing up, the answer is no.

Building the Shack and Restoring the Land

Lage: Were the younger set more shaped, do you think, by the experiences at the shack? Or did you get back there often enough that you participated?

Leopold: Since I was more or less responsible for building the shack, I was--

Lage: Tell me about that. We didn't get into that at all.

Leopold: You saw the pictures in the journal.

Lage: Yes.

Leopold: For years my father had been wanting a piece of property, and nothing quite suited him.

Lage: So he'd been talking about getting something of that sort.

Leopold: Oh, yes, for a long time. Yes. But I never really understood exactly what he was looking for, and maybe he didn't know himself. After we had the property [an abandoned sand county farm near Baraboo, Wisconsin], then it began to be clear what we were going to try to do. We were going to try to reclaim it.

Lage: At first was he talking about it as hunting cabin?

Leopold: No. It was very diffuse. We weren't actively looking, but looking back at it, it was quite clear that he was waiting for a chance to find some land he could protect, because some of his friends knew he was. I can remember the day that a friend of my father's in Baraboo, Wisconsin, phoned him--I remember the day--and said, "There's a piece of property outside of Baraboo that's up for sale. I think it might be something that you want to look at."

So on a cold, snowy day--. My sister says she was there; I don't remember. But I know my mother, father, and I, and perhaps Nina, drove out there, and it was a very bleak-looking place. There were no leaves on the trees and the snow was deep. There was this little shack half the size of this room, no larger. It was first a horse barn and then a chicken coop. We looked in it and it was piled six feet deep with manure. My mother turned to my father and said, "You're crazy. You don't mean to tell me you want this place!" My father said, "Estella, when that manure gets spread over your garden, you'll be very glad to have it." [laughter]

Lage: So he immediately saw its potential.

Leopold: So anyhow, we bought the 250 acres for practically nothing. But it was a very bleak place, I can tell you.

Lage: And when did the building take place, and what was your role in it?

Leopold: Well, I was the chief builder.

Lage: Oh, you were?

Leopold: We built a little addition which became the bunk room--just an extension of the little house. We had to repair the roof. We put in a fireplace that was on the design that my father had used when we were in New Mexico. I don't know why there was a difference, but the one in New Mexico worked, and the one in the shack that we built didn't work. We spent a miserable year with the smoking fireplace; it wasn't satisfactory. It was a dirt floor.

Finally, the next year, we said, "Let's do it properly." So on a Sunday day, my father and I went to the quarry, which was only a short distance from our house in Madison. A limestone quarry. Behind the old Essex car we had a trailer, a flatbed trailer. We went up to the quarrymen and we looked all around, and finally we walked up to the cliff. We scratched around and finally put our hands on a big rock and said, "This is the rock we want." It was about 5-1/2 feet long, nearly a foot thick, about this wide [about three feet]. We asked whether they could quarry it out for us, and they did. So with a crowbar the big rock was moved out to the trailer, and we drove it out to the shack, about fifty miles from Madison.

A few weeks later, on spring vacation, we all went out there: my sister Nina, a girl who was a friend of all of us, Mother and Father, and myself, I guess.

Lage: Starker didn't get in on this?

Leopold: He was away at the time.

We built a fireplace, and this time it was a good one. To move that rock into the shack, we built a platform of overlapping logs, and you would lift one end of the rock with a crowbar, and then the other end of the rock, and put another log under it. We finally moved that thing the ten or fifteen feet that was necessary to get it in the shack. So there's the fireplace, and it's worked extremely well.

Lage: How long did the building process take? Did you have to break the rock--

Leopold: No, we put the rock up as it was. I chipped it a little bit to sort of knock some edges off, but no, the rock was exactly the way we took it out of the quarry.

One of the stories of the family occurred when we were building the brick chimney above the new fireplace. My father was up on the roof standing on the ladder, putting a brick on the chimney. The brick slipped out of his hand and fell to the ground, and my father looked down and said, "Oops! Goddamnit!" [laughter] And that's been a family expression ever since. Oops! Goddamnit!

Lage: Was that characteristic of him?

Leopold: Yes.

Lage: And then how about the flooring? When did that come?

Leopold: Two or three years later, my mother finally said, "I want the inside of the house painted or something. It's too hard to keep clean. And besides, I want a floor." So we said, "Fine."

Lage: No objection?

Leopold: No, no. We were now ready to fix the shack up a little bit, so we put in a wood floor and we used calcimine, I guess it was, to paint everything. Not with paint, but with calcimine. That's the way it is now. It's white inside.

We never bought a piece of new lumber. If you wanted a piece of lumber, you went down on the riverbank and picked up some wood that was thrown up by the high water. So a lot of bum wood went into the fireplace because we never went to a lumberyard at all; you just went to the riverbank and got whatever you wanted.

Lage: That must give it a real characteristic air.

Leopold: Oh, yes. As a matter of fact, in the one bench that I built for the shack there is the hole that was drilled for a peg when they were floating logs down the Wisconsin River to the mill, so you knew that this was Paul Bunyan land, you see. There's the stretcher, you might say, that held the log raft together. That's one of the benches in the shack.

Lage: Did you get involved also in the restoration process on the land, or did that come after you were away from home?

Leopold: At the time that we finished the shack, finished the building, then we started seriously to plant. At that time--and you still can, you can get seedlings that were called a one-two or a two-three. In other words, one-two would be a seedling of pine that had spent one year from the time that it was germinating, and then two years of growth. So the little pines were about six inches high. You could get them from the Conservation Commission or some state agency in bundles of a hundred, with the roots.

So we started to plant. In the long run, we planted 19,000 pines. I can remember my father planting these little things that stood six inches high, saying, "The time will come when this is going to be valuable." And indeed, the white pine is very valuable stuff because there's practically none of it left naturally.

So my sister's house there in Baraboo is built out of the pines we planted. The logs are fourteen inches in diameter, and she built her house out of the logs that all of us planted when they were six inches high.

Lage: That's a wonderful feeling.

Leopold: Yes.

And then my father was very interested in prairie and the whole history of land use, so he began to move prairie plants. I can remember going out with him along the railroad track, where prairie plants had survived from the farming. He would search for a plant that he wanted and would take it back to the shack and plant it.

Since that time, my sister Nina has found you don't have to do it that way. What they do now, they first went along railroad tracks and they collected seeds. But you perhaps know that the seeds of prairie plants are extremely small. Ten could fit on the end of a pin, for example. Therefore, a special technique had to be developed for how you get seeds off these plants. Once Nina got plants started in her yard, then she got the seeds from her own plants, so that the thing has expanded now, and now they actually have enough seeds so they can actually give seed away. But outside of her house, for example, there's a place at least as large as a square block that's all prairie grass standing as high as your head, and the most beautiful flowers you ever saw. All native flowers.

Lage: I thought the introduced plants sort of tended to choke off the natives.

Leopold: That's what they had to teach themselves how to handle. So what they have to do is really sterilize the ground for one year to get rid of the weeds so they won't compete with the new prairie plants. But once that's done and the prairie plants start to come up, then apparently it goes quite quickly.

Lage: Has this become a model for government agencies and others?

Leopold: Yes. Lots of people go to Baraboo to see my sister's prairies, because now it's gotten to be a big thing. Lots of people are interested in prairie, so that there's a Prairie Restoration Research Station in Kansas that's doing it on a fairly large scale. Many students have come to work with Nina and Charlie at Baraboo to learn the techniques and to help with the reconstruction. Then those students go out and do it elsewhere. So it's now spreading; a lot of people are interested in prairie restoration now.

Lage: A lot of ripples went out.

Leopold: But when you heard about the covered wagons in 1846 going across the Great Plains, and maybe you read some of that, where they said, "And the grass stands up to your shoulder," well, indeed, my sister's grass stands up to your shoulder. You see, when you go from the hundredth meridian westward, you go first from the tall grass prairie to the short grass prairie. The tall grass prairie ends approximately at the edge of Colorado; and then from Colorado all the way to the mountains, the Colorado border going westward all the way to the mountains is short grass prairie. But the high grass prairie must have been something to see, when these billowing, waving grass stands that stood, as I say, as high as a man. It must have been quite a sight.

Lage: Really. And something to get through, also. I think it would present a little bit of barrier.

Leopold: Well, I presume so, although you see in both the Santa Fe Trail and the Oregon Trail that once the trail was made, everybody followed the same trail. Near our house in Wyoming, for example, you can see the ruts of the Oregon Trail that are two feet deep, still, just as though the wagons were there yesterday.

Publication of A Sand County Almanac

Lage: Can we talk a little bit about A Sand County Almanac and what you remember about the time your father was putting things together,

and how you helped with the publication and the final editing of it?

Leopold: It's quite clear that these essays had been germinating in my father's mind for many, many years, because after Dad died, there was found in his desk a private notebook which I had never seen, a very small notebook. Looking through it, there were phrases that he had written down in that private notebook that twenty years later appeared in an essay. Little ways of putting words together that he had thought of much earlier and then used later on. Certain essays I recall were written during the few times that my father was sick. I can remember coming in the house one day, and he had been in bed for a day, which very seldom happened. He said, with some satisfaction, "I've just finished another essay." I think that my father wrote as I do: very quickly indeed, and then spend months and even years editing, cleaning it up and making sure every word is exactly where you want it.

Lage: Did he share any of this with the family, in process?

Leopold: When an essay was done, he would make the simple statement, "I finished an essay," and the idea was, if you wanted to read it, he'd be happy to have you, but he never forced it on you, never.

Well, the essays were gradually being put together. I came back from Hawaii about 1946, it must have been, and Dad had finally gotten the essays in a manner that he felt was going to go. He sent them to a publisher; it was Alfred Knopf. Knopf wrote back and said, "These are very nice, but you ought to write more of them." My father was so angry, he could hardly stand it. This had just happened when I happened to come back from Hawaii for a trip. I was living in Hawaii at the time. I talked with Dad about it, and I said, "The trouble with you, Dad, is that you're too soft. You don't argue with these people." I said, "Why don't you let me try to get your manuscript published?" He said, "I'd be delighted."

I was in Washington shortly thereafter, and I went to see my friend, Ed Graham, who worked for the Department of Agriculture. Ed Graham had published a book on land use, so I knew he had had recent experience with publishing. I went to him and I said, "Ed, what advice can you give me?" He said, "Why don't you try my publisher, the Oxford University Press in New York?" He said that the editor's name was Philip Vaudrin.

So I made an appointment with Vaudrin and apparently I sent the manuscript to him. Then I took a special trip to New York and went to lunch with him. This is the usual way of authors

dealing with publishers. We discussed the matter, and he said, yes, he really was interested in publishing it, but there were certain things he didn't like, and that was what we were going to have to worry about.

Lage: Do you remember his objections?

Leopold: Yes, Dad's title was Great Possessions, which is the title of one of the essays. He said, "That sounds too much like Dickens." I said, "Okay, I'll make up an alternative possible list of titles."

Now, what I don't remember right now is the sequence of exactly what happened, because I think that the matter of the title came up after Dad died. Because when Vaudrin finally made up his mind that he was going to publish it, he sent a letter to Madison saying to my father that the book had been accepted and there were certain things that had to be worked out. That was the weekend my father died. So he knew that it had been accepted; that's all he knew.

So then thereafter, I was worrying now about illustrations. There was a friend of mine in Washington who had recently illustrated a book, and I asked him to make some sketches for me. They were much too modern for me; I didn't like them at all. My brother Starker had, you see, spent a good many years working in Missouri. When I was in Hawaii, I met this friend, at that time a friend of Starker's who later became a good friend of mine, Charles Schwartz. I got to know Charlie quite well when we were both in Hawaii.

It was at that time that I was now at the job of trying to get this book published, so I wrote to Charlie and said, "Would you make some sketches for me?" I had worked out a series of sentences chosen from the book that I thought could be used as illustrations. In other words, a sentence that said something that reminded you of something that might spur the artist to make a drawing. So Charlie sent me some samples, which to my mind were exactly what I wanted.

Then we made a serious effort to write down things that could be illustrated. For example, the essay about the chickadee. We picked out a sentence; say, it was about the chickadee on a sawn log. Well, that turned out to be one of the illustrations in Sand County. So Charlie did a splendid job.

Lage: What was his background? Was he a wildlife illustrator, basically?

Leopold: No, he was primarily a game manager. He was a professional game manager but has made his reputation now as a great artist.

Lage: But he knew game?

Leopold: Oh, yes, yes. Oh, yes, indeed, he did. Absolutely. No, he was a real professional. And then later when I published the second book of my father's work, Charlie did the illustrations for me again.

Lage: Who thought of the title?

Leopold: It was one of the titles that I had suggested. I was by no means happy with it. I thought there were better titles than that. But apparently Vaudrin was sort of taken with that idea. Because it's only the first part of the book that was really an almanac, which started with January, February, March. But anyhow, that's what the editor liked. So with some reluctance, I agreed to it. It's turned out to be satisfactory, but there's always been a question of whether that really would have been the best title. I'm not sure.

Lage: You don't remember the ones that you wanted?

Leopold: No, but I had a list of them. No, I don't. But obviously I was leaning toward the one that my father picked out originally. But then I had made up a series of alternatives.

Lage: Did other people review it after his death also?

Leopold: At the time that Dad had first sent it to Knopf, he had several of his students, Bob McCabe and Frederick Hamerstrom and Joe Hickey, who all had seen the manuscript. When I got it, when I was in Hawaii, there were a few notes written by those people on the margin. But the final decision as to how to deal with these minor matters were decisions that I myself had to make.

Lage: You didn't make too many changes on this?

Leopold: Oh, no, I certainly did not.

Round River: Conservationists and Hunting

Leopold: But then there was quite a different matter when Round River came along. That's an interesting story, too.

Some years after my father died, my mother said to me, "You know, there are really quite a few things of your father's that were never published, and I think you ought to publish them." Well, I was always kind of soft-hearted with my mother, so I said, "Okay, I'll try." In his files there were a series of manuscripts that had never been really finished.

Lage: He hadn't worked them over?

Leopold: Yes, but actually had not finished. They were not done.

Lage: Oh, not finished. Not even completed.

Leopold: So I took several of these and finished them myself. "Round River" was one. I didn't have to touch "A Man's Leisure Time." That was fine just the way it was. There were several others, basically where I had to add the words or I had to add the paragraphs or I had to bring it to completion.

Anyhow, the book was published, and I got the first copy. Apparently the press had written the description on the dust jacket. My mother took a look at this, and she said, "I will not accept that." I don't know where I was at the time, but I said, "Very well, Mum, I'll change it. Now, what would you suggest?" "Well," she said, "I don't want it stressed that your father was a hunter." I said, "Very well, we'll change it." So I spoke to the press and I said, "I will pay the difference. I want to have you redo the dust jacket because my mother doesn't like it. Here's the reason." Okay, they put a new dust jacket on and I paid for it. Or the royalties paid for it.

Well, shortly thereafter, when that book was published, there appeared in the Boston paper a book review that really blasted my father. It said, "He cannot be a conservationist. He's a hunter; he shoots things. He's a fraud."

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Leopold: "Aldo Leopold's a fraud. He doesn't mean anything that he's talking about. He's not in conservation at all. He shoots things." Well, I was in conversation with one of the editors of Oxford Press, and they said, "Did you see that book review in the Boston paper?" I said, "I surely did, and I was very angry." They said, "Do you know who wrote it?" I said, "No." "Rachel Carson wrote it."

Lage: Oh, my goodness. Was it not signed?

Leopold: No, she had somebody else sign it. Well, that really put me off, I'll tell you.

Lage: Did you have any contact with her on it?

Leopold: No, no, of course not. But anyhow, it's the kind of extremism that we see in many places. You think that people interested in the environment are also people that are broad minded or--. I'm not just so sure what to say, but I was shocked.

Lage: Did you run across that a lot? That kind of response?

Leopold: No, that was the only one that was important. But you see, my mother had been very smart about this. She read that sentence in the dust jacket and she said, "You take that out." But yet that still did not prevent some people from saying, "Anybody who--." Well, look at what's going on now. The whole business of you don't want to use animals for experimentation in the medical profession for the saving of human lives. And now the fur business.

Lage: But there was hunting mentioned in A Sand County Almanac, and the response--

Leopold: But you see, Round River was my father's journals, you recall, and therefore they were hunting journals like mine. So anyhow, that was part of the story.

Further Editions of A Sand County Almanac

Leopold: Then this also ought to be recorded, although no one will read it. Some years went by, and Oxford Press was not reprinting one of the books. I forget which one they weren't reprinting. I think they were not going to reprint Sand County. The editors at Ballantine Press approached the Oxford Press people and said, "We'd like to republish the book in a paperback form." Oxford Press got in touch with me and said, "What do you think?" I said, "What do they want to publish?" "They want to publish Sand County in its original form." And it was decided--and I don't remember exactly how this decision was made--that it might be better to take the best essays out of Round River, leaving out the hunting journals, and combine them with the original essays in A Sand County Almanac, and publish a new edition.

Well, now, since I was having to edit this, the question became, "How are you going to put them together?" So what I did

was to-- . Well, this is the part that's a little touchy. My former wife is an aggressive woman, and she had made up her mind she was going to edit this new edition. Well, I knew goddamn well that she didn't have the talent to do it.

Lage: Now, was she your former wife at the time?

Leopold: No, she was married to me at the time. And it was, you know, sort of a fight in the family. So she finally got her name as well as mine put on the new foreword, and that made the rest of my family madder than hell, because they didn't like her, you see.

Well, anyhow, in trying to devise a way to put these things together, it really required an entirely new lay-out for the book, because Round River was the name of the book but also the name of an essay, but it was one of the essays that I finished.

Lage: That isn't made clear, really.

Leopold: Not at all. No. Not at all clear. So what I did, was I, in order to meld them all together, I made changes in the things that I had written, without changing anything that my father had written, but changing the words that I had written in order to make this thing fit. Well, the family was madder than hell. Then there were book reviews written about how terrible it was, blaming me, you see, for rewriting everything. The family as well as friends of the family were very unhappy about the whole thing.

Lage: Did the family not realize that you had finished some of these essays, originally?

Leopold: The family tends to forget, you know. If they knew it, they didn't act that way. Anyhow, that's what happened.

Lage: How did you feel about that product?

Leopold: I was so angry that I simply said, "I don't want anything more to do with it," so I turned it over to my brother. I had been running the thing for twenty-five years and I was very unhappy about it. Because I thought it was very unfair of the family to jump on me for things that they really didn't take any trouble to find out about.

Lage: The work really enjoyed a tremendous revival of interest in the sixties. That must have been gratifying.

Leopold: Oh, yes indeed. In other words, the thing came back in a very large way.

Lage: When did the translation in Russian occur?

Leopold: Let's see. The Russian translation is dated 1980. We have just finished a translation in German. There's a translation in Chinese, and I think that we've made some progress recently in a translation in French, which I've been working on for some time. But they used the same illustrations, and interestingly, in this Russian edition they combined the two books too.

Lage: Oh, they did?

Leopold: Because the illustration--that's an illustration from Round River, not an illustration from Sand County. Since I can't read it, I don't know exactly what they did to it, but clearly they combined the two books.

Lage: Do you get any response from this personally?

Leopold: No, not very much.

Lage: The thought that it's translated into Russian is really incredible. Especially considering how environmental issues are seemingly so important in Eastern Europe and--

Leopold: Indeed. As a matter of fact, the Russians are-- Well, they're far ahead. The leadership in the Soviet Union is far ahead of the leadership in the United States as far as environmental matters are concerned.

Lage: Okay, anything else to mention on the books or the editing?

Leopold: I think that it might be said that the care and interest that my father took in writing has been tremendously influential on all of us. You can see, if you read my stuff, you can see that I'm greatly influenced by the kind of things that my father wrote about.

Lage: Even in your journals I can see similarities.

Leopold: Yes, I think so.

Lage: Have you published essays of the sort your father wrote?

Leopold: Yes, well, there are some. I think that the one that's been most widely reprinted is the one I called "Conservation and Protection."

V EARLY SCIENTIFIC RESEARCH WITH THE GEOLOGICAL SURVEY

Research in the Water Resources Division. 1950##

- Lage: I'd like to turn now to your early years with the Geological Survey. You received your Ph.D. in geology in 1950, and then went back to the survey in Washington, D.C., I understand. What was the nature of your work in those years before you became chief of the Water Resources Division?
- Leopold: I gave you those two videotapes, didn't I? [1988 interview of Luna Leopold for the U.S. Geological Survey.]
- Lage: Yes. I listened to those, and I'd like to elaborate on that here.
- Leopold: When I joined the Geological Survey, this was the first time the Water Resources Division had ever hired a man who was supposed to do something other than what other people did.
- Lage: Do research?
- Leopold: To do research, but they weren't quite sure what that meant.
- Lage: So there really wasn't a research program at that time.
- Leopold: Oh, no, there was none at all. There wasn't a research man in the whole organization. In several thousand people, there wasn't a research man.

When I came to the survey from Hawaii, and as I guess I told you, landing in Los Angeles, they didn't know what in the world to do with me. So they said, "Here's a desk," and I didn't pay attention to anybody, and I sat down and started doing my own work. So when I came back from Harvard in about three-quarters of a year, I came to Washington where I was to be more permanently assigned, and there wasn't an office for me, nor were there any instructions; nobody knew what I was supposed to do.

Lage: And who was the person who had hired you?

Leopold: The chief hydraulic engineer, Carl Paulsen, hired me.

Lage: He hired you, but without a particular framework?

Leopold: When I was in the Bureau of Reclamation before I went to Hawaii, I had in some manner or another access to people who had money. I was very much interested in what the survey was doing. Since they were right across the street from my office, I had a lot of contacts, particularly with my friend Walter Langbein, whom I was getting to know better. Walter was probably the senior mind, you might say, in the office of the chief hydraulic engineer.

So to promote the work that they were doing, I transferred some money to the Geological Survey from the Bureau of Reclamation to continue the work that was already going on in the hydrology of the western states. This went on for about a little less than a year because I was only with the Reclamation Service about a year. When I was leaving I went to call on my friends in the Geological Survey across the street, and the chief hydraulic engineer said, "We're sorry that you're leaving Washington, but if you ever want a job, we would like to have you in the Geological Survey."

So five years after that, I approached him and said, "This is what you've said." Actually, one of the chief's people was visiting Hawaii and I sent the message back through him, to the chief, and the chief then got in touch with me and it was arranged. When I came back from Harvard a little less than a year later, they didn't have any job for me. They didn't even have an office. So the assistant chief hydrologic engineer, Mr. Royal Davenport, a wonderful man--very quiet, soft-spoken gentleman of the very old school, a real gentleman--he said, "Why don't you share my office? There's an empty desk here." So I sat down in the office of assistant chief because there was no other place for me. So he went on with his business and interviewed people and talked to people, and of course I simply sat down and was quiet.

In the meantime, I had a lot of projects that I wanted to work on and was having a very fine time. I had to complete for publication a couple of papers that I had been working on connected with my doctor's thesis. That led to a lot of new investigations, but right after I got back from Harvard, in June of 1950, I was in touch with my closest friend at Harvard, John Miller, who was at that time at Penn State, and said, "Let's go

to the field together." So that summer, the first summer with the survey, I went to the field with John Miller.

Well, we had a wonderful time. We were investigating the whole question of the effect of changes of climate on the river valleys of the western states, especially Wyoming. When we got back from that summer, the question that kept sticking in my mind was, why does the river have such a width? Because the width is really what was going to determine how the terraces were going to develop. So I picked up the question of why a river is as wide as it is. This turned out to be a very fruitful question indeed. A question that really no one had ever asked before.

The next thing that happened was that-- Well, there were two really important aspects of this. In the first place, because I'd just come from five years of being a professional meteorologist, I visualized everything in terms of the atmosphere and the structures that we see in the atmosphere, which you now see in the form of fronts on a map on television everyday.

I'm now going to transfer my meteorological training to the river system, because now we're still talking about a fluid, you see. The air as a fluid has certain shapes and forms and does certain things in a very consistent manner. Water is a fluid also, and therefore there must be some relationship. So I started to pick up the ideas that I had absorbed as a meteorologist and started to apply them, to ask the questions in terms of--

Lage: Ask the questions of the river that a meteorologist asks of the atmosphere?

Leopold: That I had been working on as a meteorologist. For example, one of the major tools that was being developed at that time was called an isentropic map. That is a map of equal entropy. Basically, equal entropy means, in very shorthand, no loss or gain of energy. Well, I said, now, if that actually accounts for these waves that we see in the atmosphere--at least one aspect of the waves--is there something comparable in the fluid of water? Yes, indeed, there is something. So that was the kind of idea that I had.

So really, there were two things of great importance: one, having worked on terraces that first summer the question was, why is a river as wide as it is? The second, transferring the ideas that I had picked up as a meteorologist to ask the same kinds of questions of the river system. That really started the direction of my research. As a result, the study of river terraces has

been a preoccupation of mine all my life, starting with that particular summer.

Lage: Were there people in the Geological Survey that encouraged you along these lines, or were you really just a sole figure then?

Leopold: I was really quite alone, except that I could walk across the hall and talk to Walter Langbein. But Walter wasn't working on research as much as he was working on primarily the--. Most of his time was taken up by the things that were asked of him in an administrative way.

Lage: What was his position at that time?

Leopold: I'm not even sure what it was called. He was certainly called a hydraulic engineer, but there was no particular position. He was simply on the staff of the chief hydraulic engineer.

Lage: But he had administrative responsibilities?

Leopold: He was always called upon because he was good at it. And in his spare time, basically, he was working on some very important hydrologic problems in research. Then he was also very important in many other ways. When I became chief of the division, I appointed Walter chief scientist. But he was great at training people, so that people would come in from the field and spend six months working in his office, more or less under his direction, and he really was the leader in the development of several of the most important research people that later on became prominent in the research game.

Lage: This is even in those early years?

Leopold: Yes, this was still in those early days. So that I would go across the hall and there would be someone in Walter's office that was basically working under him on some research project. For example, they worked on such problems as how much water is lost by evaporation from a stock pond. This is a research project. How do you compute it?

You compute it among other things by measuring the radiation from the sun. You also can compute it by measuring the change in elevation of the water surface. But the measurement of water surface was not enough because you didn't know how much leakage there was. So then the question was, can we compute evaporation by measuring the sun's radiation? Which means that you have to know about the albedo, that is, the amount of reflected energy. You have to know what kind of wavelengths are coming in. So then

you have a whole new series of instruments. You're now talking about pyroheliometers set up near the water surface.

Later on this became terribly important when they started this big, very expensive survey at Lake Mead, because Lake Mead has an evaporation of more than three feet a year. That's a very large amount of water. So with the projects that Walter had been working on with the people that were sent to help him or to work under him, they had already developed a method for doing it. So when they got to the big reservoir, they now had a theoretical method and they knew how to make the measurement.

Lage: Did they choose this research subject because there were problems with stock ponds, or did they do it to study the pure problem of evaporation rates?

Leopold: Both. In other words, the whole question of evaporative loss in the western states where water is short is extremely important. They worked on the stock ponds because they could handle it. They were small enough to really make direct measurement.

So anyhow, here were things going on that were really research but not called that, and then nobody had been assigned to research except I. I could see that there were many potentials, because here were people being trained in research techniques, here was one of the great minds in the whole organization that was having to spend his time writing memoranda for the chief.

Lage: I don't like to keep interrupting, but questions come up. Were the people he was training Ph.D.'s?

Leopold: No, no, not a single one of them. No, not at all.

Lage: What would their background have been?

Leopold: They were usually engineers. For example, there was the highly qualified man who's been working with me off and on, especially in this court case in the last year, David Dawdy. He got a start working under Walter Langbein. Earl Harbeck was the man who really developed the theory that was used in the measurement of evaporation. There were flood specialists. Those are the two that I remember that were working under Walter during that first couple of years.

Lage: So they didn't necessarily have the kind of research background that they needed for some of these questions?

Leopold: No, but they were developing it. They were developing it, believe me. They all turned out to be very highly qualified and important research people.

Research with John Miller on Influence of Climatic Changes on River Valleys##

Lage: I want to talk more about your research during the years before you became chief of the division. Can we talk more about some of the important research like your work on hydraulic geometry, and try to make that understandable?

Leopold: The thing that made the difference was that right there in the hall where I worked, just two doors down from my office, were all the streamflow records for the whole United States.

Lage: All this data collection.

Leopold: All these data, you see.

Lage: What kind of data did they collect?

Leopold: They measure the stream, so that on a certain day they had a current meter out there and they were measuring the velocity. So here was this whole room full of tabulated data from the field that no one had ever touched.

Lage: If the data wasn't interpreted, what was it collected for?

Leopold: The amounts of water were published, but the details of how they got there, the details of the stream itself, were not published. So they told you on August the 1st there was so much water in such and such a stream. But the data that they measured in the river had never been published, you see.

Lage: I see.

Leopold: So here were all these direct measurements, field measurements, that no one had ever done anything with. So I said, "I'm going to start fooling around with this."

Well, I left Harvard in May, 1950, and I told you I had made a very good friend at Harvard, John Miller, who took his degree at the same time. John was younger than I but was a geologist who had worked for many years in New Mexico. When John graduated at the same time I did with his Ph.D., he immediately was offered

a job as assistant professor of geology at Penn State. We were both very glad to get out of school, and I said, "John, let's spend the summer together." I said, "I'll meet you in Sundance, Wyoming," on a certain day.

John and his wife, Laura, drove across the country, and they arrived in Sundance. They were very apprehensive about whether I really meant business. But they were happy to start the field work themselves. So when the train pulled up-- . In those days you could take the train. I took the train from Denver to Sundance. When I stepped off the train they were quite surprised that I really was there.

So I said, "Do you still have a hotel room?" "Yes, we've got a hotel room." I said, "Okay, I've got to put on my field clothes." So I went to the hotel. Sundance was about the size of Pinedale; not even a paved street in the front. I went in the hotel and I put on my blue jeans and my boots. I had a great big Stetson about this high that was twenty years old. Apparently I came out of the hotel, ran down the steps to the middle of the street, threw the Stetson up in the air, and yelled, "Hah! the field!"

So we started out. We had a wonderful time; we learned all kinds of things. We had one ball out there.

Lage: Were you focused on a particular thing?

Leopold: Yes. Because John and I were both interested in the things that Professor Bryan had worked on all his life and that I had found in New Mexico.

Lage: The Pleistocene.

Leopold: Yes. We wanted to make a survey of Eastern Wyoming to see what the climate was in the latter part of the Pleistocene and the Holocene in Eastern Wyoming. That was our plan, and indeed, we were very successful. So one of the papers that we had written during that time before I was made chief was a paper on the climate, and thus the geology, of the rivers of eastern Wyoming ["A post-glacial chronology for some alluvial valleys in Wyoming"]. I published that in the main Water Supply Series of the Geological Survey.

But when I finished my paper on "The hydraulic geometry of stream channels and some physiographic implications," that later became famous, I had recommended that it be published as a professional paper. Now, it turns out that the professional paper series was primarily a series that published geological

material. The water supply paper series was primarily publishing water material. When the chief hydraulic engineer received my manuscript for permission, with the recommendation that it be published in the professional paper series, certain people came up to him and said, "He can't publish that in the professional paper series because that's not our series. That's the geologic division."

So I went to the chief and I said, "Sir, I've written two papers. This was a paper on geology which was written for the hydraulic engineer. So my geological paper on western Wyoming had to be published in the water supply paper series. Here is a paper on water which I want to publish for the geologist. It's going to be written in the professional paper series." He said, "That's a good idea." So that's the way it turned out.

So I started, then, this cross connection, that I later expanded, for writing papers on water for the geologist and writing papers on geology for the water people. So this began to change the way that people were looking at what we were all supposed to do.

Lage: I noticed a paper on nineteenth-century vegetation in the Southwest ["Vegetation of Southwestern Watersheds in the Nineteenth Century," 1951].

Leopold: That was part of my doctor's thesis. What I was trying to do there was to determine if old photographs recorded conditions different from those observed today. By reading journals of early exploration and collecting early photographs--I was still working on the problem of erosion, you see.

Lage: So all of this was related to the problem of erosion.

Leopold: Yes. That's right. And then one of the papers that was in my doctor's thesis again became a famous paper because we had talked for many, many years about climatic change, but no one had ever computed quantitative values for climatic parameters during the glacial age.

In another paper I found something that no one had ever expected. There's a very famous geographer by the name of Thorne Thwaite who had written a paper saying, "The data do not show any climatic change in the latter part of the nineteenth century and therefore the erosion problem must be due to man."

What I did is I showed that the averages didn't change, that was true, but the rainfall intensity changed. My paper on

rainfall intensity, then, was a way of explaining how a particular aspect of the climatic change would have been the most important in the erosion problem. No one had ever seen that before. ["Rainfall intensity: an aspect of climatic variation."]

Later it's been confirmed by many, many people, and now it's agreed upon that the climatic change in the last part of the nineteenth century was not a change in the annual rainfall but a change in the type of precipitation.

Lage: How did you determine the intensity of rainfall in the past?

Leopold: Well, I saw that as a climatologist, you see. I mean, after all, I was coming from meteorology, so I had ways of thinking about it that other people hadn't thought of at that time. I took the oldest rainfall record in the United States--again, you see, New Mexico. The oldest rainfall record in the United States started at Fort Marcy in Santa Fe in 1846 when General Doniphan first got there and started the Mexican War. They started to collect rainfall records. So I took those records and I counted the number of days of different amounts of rain, and showed that the frequency of high rainfall changed in the latter part of the nineteenth century.

So here were a series of papers all dealing with the interaction between meteorology and water and geology, which were published at the time before I became chief.

Genesis of Hydraulic Geometry

Lage: What about hydraulic geometry? Tell me what that is.

Leopold: This is a good question. It started this way. John Miller and I were out in the field in eastern Wyoming trying to explain to ourselves how the climate of the Pleistocene had changed the landscape. We saw evidence that the landscape had gone through climatic change and caused deep gullying which occurred in the period from 1200 to 1400 A.D., and then the gullies filled up, and then they repeated themselves at the beginning of the present century. This had first been demonstrated by Professor Kirk Bryan.

Lage: So you saw this in the geological record of the stream bed?

Leopold: Yes. The question then came up, in many different ways, why is the river as wide as it is? That was a question that I posed for

myself which turned out to be a very important question. As I often tell students later, "The important thing is not how you do it but what question you choose to work on." That, I maintained, was a very important question. It affected everything I did for many years later. This is the kind of difficult question that the present students are missing. They are not paying attention to the really important questions. They are taking things that are too small and too easy.

Lage: Why did that question occur to you? Do you remember?

Leopold: Because here John and I were looking at these gullies, you see. And we were saying, "Why should they be as wide as they were?" We were looking at it geologically, and then I started to ask myself, "Hydraulically, from the standpoint of the flow of the water, why is it this way?" So I got into a whole lot of literature about what the English engineers were doing in India and how canals were designed and that sort of thing.

Then I remember the day I walked into Walter Langbein's office across the hall, and I said, "Look what I found." I said, "Do you realize that the relationship between the width of the river and the discharge follows in a certain mathematical formula?" "No," he said. "That's really interesting." I said, "Did you know that that's exactly what the engineers found in India in 1890 when they built canals?" So then we began to see that we were talking now about how stable channels operate. Nobody knew why, and they didn't see all these interconnections, and then I started to put these interconnections together.

Lage: As you speak, it's so obvious how your kind of unique education really came together.

Leopold: The main thing is that the combination that John Miller and I were working on--. We were looking at the geology in the field. We were interpreting the geology in terms of what had been changing in the last ten thousand years. That had certain climatic and hydraulic relationships, which becomes a water problem. Being a meteorologist, I was looking at it from the standpoint of climatology and from the standpoint of hydraulics and from the standpoint of climate and from the standpoint of geology. That kind of combination is the kind of stuff we need.

Lage: And John had been a geologist, or did he have a background--

Leopold: Oh, yes, John was a tremendous help. We knew different things. John had been mapping in northern New Mexico and was a real expert in geologic mapping. But John also was an expert in soils. He was a very highly trained chemist with particular

emphasis on soils. Soils were one of the things we were using as a measure of climate, so that his knowledge of chemistry and soils, and what I was bringing from climatology and hydraulics, we were combining into a way of looking at things.

For example, we wrote a paper together called "The Role of Paleosols in Climatic Interpretation," something like that. Paleosol was an ancient soil from a different climate. So when we were in eastern Wyoming, we were measuring the soil profile, in his terms; in other words, we were actually measuring the amount of gypsum and the amount of calcium carbonate deposited in the soils. So it was a very good combination.

And then later on--. My whole career has been characterized by finding somebody who knew something that I didn't know, and finding ways to work closely with that person, so that we put our heads together and did something that neither one of us could have done alone. My later relationship with Walter Langbein, who is a real genius, was of the same sort.

John and I had worked on several things together. We wrote this paper on eastern Wyoming. We wrote the paper on paleosols. We wrote the paper, which became very well known, on ephemeral channels. The channels that are dry most of the time and flow only during rainstorms.

Lage: What was that one called?

Leopold: That was called "Ephemeral streams: relation to the drainage net." That was a professional paper of the Geological Survey.

Further Collaboration with John Miller: His Untimely Death and Special Qualities

Leopold: When Kirk Bryan died--I was his last student; he died the year that John and I took our degree--John Miller was asked, after kind of an interim, to leave Pennsylvania State and go to Harvard to take Kirk Bryan's place, which he did. So the student of Kirk Bryan now became Kirk Bryan's successor. He came up for tenure. A great geologist in the Geological Survey was on the visiting committee, and he came to see me. He said, "You've written these papers with John Miller, and Miller's coming up for tenure. We can't tell what his contribution was because you're the senior author in most of these papers. What did he do?"

Well, I tried as best I could to explain, but they didn't promote him at that time. I went to John and--. We were the closest friends. I said, "John, you and I can't work together for a while." I said, "You've got to wait until you've written some papers all on your own and you get tenure, and then we can start working together again."

So many years went by, and John was on his own, obviously proved himself to be an extremely good scientist. And then we started to work again in about 1958 after he had published several very important papers of his own. We started a project in New Mexico that involved a lot of things. We decided that we were going to find how individual rocks moved on the stream bed, so we started the business of painting rocks, which now has spread all over the world.

We would take rocks off the stream bed and take them to our truck, weigh each rock, paint them orange, and then paint the weight of the rock on the rock. Rocks always have slightly different weights in grams; for example, 5,212, there's not going to be another rock of exactly that weight. So that when you picked up the rock after it moved, you can look at it and say, "Yes, I know where that rock came from. That's Number 5,212, and it came from so-and-so a place."

So we laid out these rocks in different patterns to find out what rocks moved and under what conditions they moved. We were well into this procedure. We had developed a lot of new ideas on how to measure these things.

One of the things that we did was I invented the system of what I call bank pins. I'd take a steel reinforcing rod and drive it into the bank of the stream and let it stick out just two-tenths of a foot. Then after a year you'd come back and measure it, and if it were sticking out this far you could then know how much the erosion was.

We did the same thing with vertical rods. We invented the things which are now called scour chains, where we dig a hole in the bed, and--. People had tried this before, but they had failed to do one important thing that we did that was right. You had to have the links of the chain large enough so the sand would get in the link. So the links that we used were about this size [1 centimeter]. We'd dig a hole, take this length of chain, tie a rock on the end, put it down the bottom of the hole, and then holding the chain vertically, would fill the thing in and lay the chain on the ground. Then when the stream came along and washed away the sand, the chain, now, which formerly was like this, would now bend here and now be strung out at some depth. And

then, since we knew exactly where that chain was because we could stretch a tape from our benchmark, we could dig down and find out how deep was the bend in the chain. The bend in the chain shows the lowest elevation of the stream bed during scour by flood.

Well, that's in the middle of what we were doing. At the end of the summer in 1961, we were well into this business. We had painted hundreds and hundreds of rocks and we had cross-sections everywhere and maps and the whole thing very well done. John had to go back to Cambridge, so I put him on the plane on a Friday afternoon. Saturday morning I started out on a field trip in Colorado. On Tuesday I was up in the mountains and I was pulling a car out of the mud or something, a place in the high mountains. A car came by and said, "Your name Leopold?" I said, "Yes." They said, "The sheriff's looking for you." I said, "What's the trouble?" They said, "I don't know, but they're very anxious to get in touch with you. You'd better get into town and call the sheriff."

So I drove down to the nearest town and called the sheriff, and he said, "Your office in Santa Fe is looking for you. There's been a terrible accident." John had died.

Lage: Oh, no. How sad. I didn't realize you lost him so--

Leopold: He got bubonic plague from our work in New Mexico.

Lage: And just like that?

Leopold: Yes, and he was dead in two days. And then the question was what had happened. Well, it turns out that bubonic plague is endemic there, and he was bitten by a flea. Had he been taken in Santa Fe, I'm sure they could have saved him.

Lage: They would have known.

Leopold: When he got to a small hospital in Cambridge, they'd never heard of it. So instead of treating him, they let it go for two days, and in two days he was dead. So anyhow, it was a terrible, terrible thing.

So then I had a long bout with the Center for Disease Control in Atlanta. Over a matter of about ten months, meeting again with all kinds of doctors and stuff, I finally persuaded them that the mark that he had on one hand was indeed a flea bite. Then it was proven all over again that the fleas in the area where we were working had bubonic plague, so it made a tremendous difference to his family because Laura, his widow, got a very good pension that's lasted--kept her alive all her life

now. But it was a very sad thing because he was an extremely competent geologist.

Lage: So there was a question even after he died that--

Leopold: Well, we had somehow to prove in one manner or another-- . Apparently the people that gave out the pension, they insisted that there be some physical evidence that he died on duty. Well, on duty he was when we were together, but he died in Cambridge when he was not on duty, and I had to prove that he contracted a disease which killed him while he was working for the Geological Survey with me. Anyhow, we finally persuaded them to do so.

Lage: It's so sad.

Leopold: But John was a different sort. He was the most competent geologist I've ever worked with. For a man of his age, he really was a wonder.

Lage: What qualities did he bring to it that makes you characterize him that way?

Leopold: Well, in the first place, he was an extremely hard worker and loved doing it.

Lage: He liked the field the way you did?

Leopold: Oh, yes. Oh, yes. And he could walk your pants off. We went to some of the toughest places that I've ever been. I used to get really quite unhappy with John because he was always going way ahead of me because he could walk faster than I over rough country. God, he was a wiry fella. That's his picture there, the center one.

Lage: Oh, yes.

Leopold: Up on top of the peak. He was a very, very competent geologist. We had an awfully good time together. We just loved what we were doing.

Lage: He seemed to have an intuitive quality of mind.

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Leopold: I think that's the thing that characterized both of us, as a matter of fact. I would say different than anybody I've ever worked with. None of my students seemed to be able to have the same thing. There are some very brilliant ones, but I think

that being able to see intuitively what's immediately in front of you is a characteristic that's rather rare. And John had it.

Lage: And you can't say what it comes from. In your case you could attribute it to the training of your father and the way he looked at the world and nature. Or is qualities of mind?

Leopold: I don't know how to describe that. It's being able to think in dimensions of time and space, to think about the geologic setting, the geologic history, and the present processes simultaneously. I think that's probably the thing that distinguished him.

Fluvial Processes in Geomorphology

Leopold: So anyhow, right after that, John and I that summer, as a matter of fact, had been sitting under a piñon tree there outside of Santa Fe writing the outline of the book that we were to write.

Lage: On?

Leopold: We were at that time writing a book, but we had just gotten started. John had a draft of one chapter when he died. Well, I made up my mind on two things. In the first place, I was going to publish that book, period, which I did then. And second, I was going to complete the work in New Mexico, which did occur too. Both of them were accomplished in good time. And the book, of course--

Lage: Which is the book?

Leopold: Leopold, Wolman, and Miller. This book has been the most important geomorphological book up until very recent times. It was the standard for the whole world until, oh, in the last five or seven years. A lot of new books have come out, but this for many years was the important book in geomorphology.

Lage: Fluvial Processes in Geomorphology [San Francisco: W.H. Freeman, 1964]. Who was Wolman?

Leopold: Gordon Wolman also was taking his degree at Harvard when we were both there. He finished his degree a couple years later, and he and I worked together very closely for quite some years, so that when John died, I asked my friend Gordon to help me finish the book.

- Lage: You made the statement--maybe it was in the video--that you were responsible for bringing geomorphology to the Geological Survey. What does that mean, and how did it happen?
- Leopold: When I joined the Geological Survey in Washington, as I told you, and people asked me what I worked on, I said I work in geomorphology, and they said, "What's that?" No one in the survey had ever heard the term.
- Lage: Now, what is the term? What does it mean?
- Leopold: Geomorphology is the--. The morph- is "the form." "Geo-," "of the earth." And "-ology" is "the study of." Geomorphology is the study of the earth's surface forms, and by forms we mean processes as well.
- Lage: Why wasn't that a part of the Geological Survey?
- Leopold: Because the people in the Water Resources Division were interested in water. They didn't know anything about geomorphic problems, you see.
- Lage: I see. So the focus was much narrower.
- Leopold: Yes. And that's what I expanded. So anyhow, what Walter Langbein and I did, as soon as I became chief and we had some money, we said, "We're going to hire people in a whole lot of fields to do things in a research way in the field of water." Within about six or seven years, what we created was the most important research organization in the world, in the field of water. It became very famous. It became famous because primarily everybody was looking for the professional papers of the Geological Survey to see what was new. That's where we were publishing.
- Lage: I think that's a good place maybe to stop for today.

VI A NEW DIRECTION FOR THE USGS WATER RESOURCES DIVISION

[Interview 4: January 17, 1991]##

USGS Directors Wrather and Nolan

Lage: Today, after a long break in our interviewing, we're going to look at the U.S. Geological Survey in some detail. We talked last June about your early research, and I thought we'd turn today to your appointment and work as chief of the Water Resources Division.

Leopold: We talked about my paper on river channel geometry, which everybody recognized at the time was going to be an important paper. Presumably, I gathered that the various papers that I was working on must have been called to the attention of the director [of the USGS]. I didn't see the director probably once or twice in that whole several years.

Lage: Who was the director at that time?

Leopold: Thomas B. Nolan was the associate director. But the director at that time was William Wrather. Bill Wrather came basically from industry, but he was a very good spokesman for the survey and a very understanding man. But he was the kind of leader that was basically a political--not political in the ordinary sense, but the public relations man who could explain to the Congress what we were trying to do, and he was very good at that. He also was very interested in these problems, but not in a detailed way. He was not a research person himself.

I remember one time, for example, Dr. Wrather asked me to go with him and the chief of the Groundwater Branch, to go over to New Jersey to visit the famous meteorologist, Warren Thornthwaite. He was interested enough, you see, to actually go out and talk with this famous meteorologist, because Thornthwaite at that time was working on the problem of determining how much

water they needed for irrigation for crops in the East. Later on he turned this into a very lucrative business by basically selling his services to people like the people that made frozen peas, for example. I forgot what you call them, but anyhow, to determine how much water the plants needed, he was turning his meteorological background into something very practical.

In this way, therefore, I got to see a little bit of other research people. This is the kind of thing that Bill Wrather did, but he was not really the one that was really running the details of the survey. It was the associate director.

Lage: So Nolan, even before he became director, was--

Leopold: Yes. And Tom Nolan had been a very loyal assistant director for, oh, I don't know how many years, but it must have been at least a half a dozen years, I suppose.

The time came when Dr. Wrather was going to retire. Now, up to that time, the customary way the director of the Geological Survey was chosen was for the Department of Interior to turn to the National Academy of Sciences and say, "Would you make recommendations?" Usually the way that worked was that the president of the National Academy would set up a committee who would study the problem, and they would make a recommendation of somebody's name to be the director.

Tom Nolan was obviously a choice. He was a member of the National Academy of Sciences himself. He was a member of the American Philosophical Society. He was a very important geologist and a research man in his own right. He had spent every summer in Eureka, Nevada, working on a mining district there, so he knew research and he was a very good working geologist in his own right. So the Academy--and I don't know the details of this, but that was the way it was done in those days--the Academy must have recommended Nolan, and Nolan was appointed.

Well, then as soon as Nolan was now director, now things were going to start to change, because now for the first time in a long time since-- Well, let's see. First there was John Wesley Powell, and then came-- Gee, I can't remember the names of all of these people who had been in the director's job. But in recent years Tom Nolan was the first one who was actually a research scientist himself and a member of the Academy. But once he got to be director, then he started to shift things around.

Assistant Chief Hydrologic Engineer: Initiating Controversial
Changes in Budget Process

Lage: You saw an immediate change?

Leopold: Oh, yes. A change immediately. Yes. He right away called me in and said, "I want you to be the assistant chief hydrologic engineer. I'm going to bring in another geologist"--whose name was Raymond Nace--"from Idaho to be the other assistant chief." Royal Davenport actually remained the assistant chief hydrologic engineer, so there basically must have been three of us. I don't remember exactly how the thing looked on an organization chart, because Mr. Davenport was still very important in administrative ways.

What Nolan wanted us to do was to think through how the Water Resources Division ought to be changed. So I was given the job of dealing with money. I was really the budget officer. Ray Nace was given the job of essentially the operational officer. Just about that time, Royal Davenport retired, also before Paulsen retired, I think. I think that was the sequence. Anyhow, it turned out that under Mr. Paulsen, Nace and I were the two assistant chiefs in charge of operations and budget respectively.

Well, it had always been the idea that in splitting up the money that was available for the work of the Water Resources Division, the people that really controlled it were the chief of the Surface Water Branch, who at that time was Joseph Wells, Joe Wells, and the chief of the Groundwater Branch Nelson Sayre. They really, together with Paulsen--the chiefs of the two big branches and the chief hydrologic engineer--they basically decided what they were going to do, and the matter of water quality and the other aspects of water simply were not given much consideration.

I took a look at that, and I said, "All that's doing is perpetuating what we're doing." Everybody was so concerned with the cooperative program because half of the survey's money was coming from the states under the cooperative program. The states would say to the chief of the Surface Water Branch, "I want a gauging station at such-and-such a place, and we'll pay 50 percent," and the Congress gives the USGS the money to pay the other 50 percent.

Lage: So that determines the program.

Leopold: That was running the whole survey, because a large part of the total money was coming in the form of the cooperative program. The cooperative program was just data collection.

Lage: Did the cooperative program involve both the Surface Water Branch and the Groundwater Branch?

Leopold: That's right, yes.

Lage: Were both of those part of data collection?

Leopold: Yes, because as I say, the regular cooperative program in the Groundwater Branch was to make a study of a certain county. But there wasn't any research in it; there was measuring wells to try to determine what was the nature of the aquifer and how much water was being pumped out. But there was not any research in it. And that was the problem, that there were many important scientific aspects of groundwater that weren't even being looked at.

Lage: They were collecting data in case somebody wanted to look at it.

Leopold: No, the state wanted it. They said, "All right, now, we're going to make a study of Contra Costa County." All right, the state puts up half of the money, and the survey's told to go out and measure the wells in Contra Costa County, and the survey comes up with a report called "The Groundwater Resources of Contra Costa County." But all of the county reports were pretty much the same. There basically was no research part of the organization.

The other problem was that the groundwater people gave everybody the impression that they were the real scientists, because they were not ordinarily just making measurements of the rivers the way the Surface Water Branch was. They were the scientists, and they were the only scientists. They felt that they were the chief scientific branch.

Well, that really wasn't quite true because there was a lot of good work going on in the other branches, but they were always sort of pushed aside and the groundwater people felt that they were the top dog.

Lage: Were there other branches other than surface water and groundwater?

Leopold: Yes, well, there was water quality, but they were sort of the stepchildren.

When the chiefs of the branches heard that I was going to decide where the money was going to go myself, instead of letting them decide, there was a great outcry. They were now essentially losing their ability to determine the direction of the survey. I said, "Now we're going to start doing this a little differently. We're going to decide. I'm going to decide." So Nace and I then sort of decided what we were going to do. But at the same time, things were not going to change very fast because the people really in charge still were the branch chiefs, and they were not happy about the redistribution of money, but nevertheless, they were the ones running the program.

Lage: When they got the money, they would decide what to do with it.

Leopold: Yes, that's right. And it was, of course, more or less a continuation of what they were doing.

Now, there are a lot of details about these matters that I don't remember very well, but that was the gist of it. This lasted for, I suppose about two years. Dr. Nolan knew that Carl Paulsen, the chief, would retire, so he was simply biding his time, I think, in order to make more drastic changes. But he was basically getting two people--Nace and myself--to be in a position to start making the changes that the director wanted.

Accepting the Job of Chief Engineer and Director Nolan's Mandate for Change, 1957

Leopold: Well, came the time that Mr. Paulsen retired. He was a grand guy. He was a very friendly man, very easygoing, not a terribly good speaker but he knew everybody by their first name and he had been in every office in the survey and he knew all the people in the field, and everybody loved him. But he was basically an administrator of the program the way it was. He was a very popular man.

And then when it was announced that I was going to be appointed the chief, everybody could see that as something quite different. Because in the first place, I wasn't known; I didn't know these people.

Lage: You hadn't been out visiting the field.

Leopold: Hell, no, I didn't know anything about that. But I remember the day that the director asked me to be chief. I asked my friend Thomas Maddock who had been my mentor when I--

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Leopold: Partly through my influence, Tom Maddock was hired by the Bureau of Reclamation, and of course he was a very experienced engineer and a very close friend of mine. I asked him to come see me, and he did. I remember when he walked in the front door of my house, he said, "Well, Luna, you're a big boy now. Yes, of course you're going to take this job."

Lage: You had some doubts of your own?

Leopold: Oh, yes, I didn't want to do it. In the first place, I didn't like the administrative work of being assistant chief. I really was more interested in my research. But it was immediately apparent that there were things that could be done.

Now, what happened then--I tell it this way but it may not be exactly true. When the director talked to me, he said, in effect, "I really don't know what needs to be done in the division, but the division has got to change."

The Water Resources Division was not like the Geologic Division, where over many years they had the custom that if somebody was a branch chief, he was a branch chief only a certain time, and then he was allowed to go back to the field to do his geology. So there was continual rotation. But these people in the Water Resources Division had been the branch chiefs all their lives, or many, many years. There was no rotation at all. And therefore, you see, there was no way of the things getting changed. Nolan didn't say that exactly, but he implied--and I had learned enough now about the divisions to see what was going on. The implication to me was clear: that the business of having no people moving around and no new ideas coming in over long periods of time was not working.

Lage: Did he tell you in general way what he wanted to see?

Leopold: No.

Lage: He didn't say, "I want more research, more basic research"?

Leopold: No, no. He said, "It's got to be improved, but I don't know exactly how to do it. You'll just have to figure it out." I said, "I'm going to have to have some money." He had gone to the Congress and put it in the budget, which I didn't know about. He said, "All right, I'll give you \$2 million." That was a lot of money in those days. He had apparently rearranged the budget; as soon as he got to be director, he asked the Congress for more

federal money to carry on the kinds of work that he had been doing in the Geologic Division, and he was going to divide it among the various divisions. There were three divisions, you see. There were actually four. There was the Topographic Division, which makes maps; the Geologic Division, which has always been known as a scientific organization; the Water Resources Division, which traditionally had been known as the people who measure water but which now was going into scientific research; and the so-called Conservation Division, which dealt with oil and gas matters.

Allocating an Increased Budget: New Programs and Personnel

Leopold: So all right, now I have some money, which I had never had available before. Federal money. It was not dependent upon the states.

Lage: I see. So this made it very different.

Leopold: Oh, it made a lot of difference. So I called Walter Langbein in. I had just moved into the chief's office. I said, "All right, how are we going to spend this money?" We started laying out pieces of paper, I can remember, on a long table. Walter said, "I would like to have a program in glaciology." I said, "Great. That's another aspect of the survey that we've never done anything about. Let's go into glaciology."

I said, "I want some hydraulic work done on the kinds of streams that I've been working on in the West, which I would call the problem of the alluvial stream. We know quite a lot about the hydraulics of streams in the East, but there are a lot of hydraulic problems of a new sort in the Far West."

We decided that we wanted a program of education. No schools were teaching hydrology. We wanted a program in groundwater mechanics, including what later turned out to be the great work of Herb Skibitzke, my friend, who really invented the whole business of modeling groundwater, first with electrical analogs and then later on computers. I wanted something on chemistry of water beyond what they were doing, and pollution. There was no program in pollution. So this was how it went.

But anyhow, we divided up the money, and now we had to find people.

Lage: This is a major thing, deciding on these new programs. Did you get other input or spend a long time, or had you had this in mind?

Leopold: No, Walter and I did it in a very short time. No, we knew about what we wanted. We wanted to spread the division out into a research program that involved many aspects of water that had never been touched. I said, for example, I wanted a hydraulic laboratory. Well, this didn't just occur overnight. The decision about what we were going to do with the money was done very quickly. Now we had to manage, you see.

Hiring and Retraining Research Staff

Lage: You had to have personnel.

Leopold: Yes, now we had to get personnel. So the first thing I did was I said, "All right, I'm going to use some of this money to send people back to school." Well, this was a hell of a big change. So those people that appeared to have the qualifications, I said, "Okay, you're a GS-12," let's say. "You're on permanent duty. You're a civil service employee. I'm going to assign you to such-and-such a university and you're going to work there on a degree." The men were then transferred. The whole family was transferred to the place that the man was going to go to the university.

Lage: Did people apply for this, or did you go to them and say, "This is--"

Leopold: No, I was picking them out.

Lage: Were they receptive?

Leopold: Oh, yes.

Lage: This was not something you had to force on them.

Leopold: No, but the thing is that there was nothing in the federal government that allows you to do this. I was doing it surreptitiously. Later on, after I had this whole program started, then they passed a law in Congress that allowed you to do this. But at that time there was no law. This was simply my idea: "This is what we're going to do."

Lage: Were most of these people engineers?

Leopold: They were both engineers and geologists, and some chemists. How many people? I suppose at least a dozen, I guess.

Then I started to pick up people that were already finishing their degree. For example, what were we going to do about glaciology? Well, through my contacts at Caltech where I had been a visiting professor before, I knew that there was a young man just graduating from a Ph.D. program in glaciology. His name was Mark Meier. I got in touch with Mark and I said, "You're just getting your degree. I wonder if you would want to come and be our glaciologist." He had many other opportunities, and I said, "One thing that you can be assured of is that your first year, you're not going to be asked to do anything except finish what you're already doing. You have to finish up your thesis and get it ready for publication, so anyhow, you do exactly what you want. After that, then we'll talk about what kind of a program you want."

Well, that sold him. In other words, he didn't have to go to a university and start teaching. He could continue his work on the thesis.

Lage: And design his own program.

Leopold: Yes. I have found--and I think this is absolutely true--when you hire a Ph.D., the first year of his life after he gets his Ph.D., he's going to continue to work on the subject of his Ph.D. This is absolutely universal. I know of practically no exceptions.

Lage: To get it ready for publication?

Leopold: And you know, to finish up, because you've been immersed in it, you see, under the university. Now, the first thing you want to do is get the thing tied up. Well, this made a lot of difference. But if you hadn't gone through that experience, you wouldn't know that this is the way people think. So I got a lot of people by saying, "In your first year, you just work on what you want to on your thesis. On the subject of your thesis." In other words, extend it in some manner or another. That's what I did. My first couple of years in the survey I was working on an extension of what I'd done for my Ph.D. thesis.

I don't know where I found all these guys, but most of them came out of the survey itself.

Lage: Most of the ones that headed up the new programs?

Leopold: Yes. And about a third of them, I imagine, were hired anew from the outside. For the people on the outside, I didn't have anybody unless they had a Ph.D. I was determined that the only way you were going to get into the scientific work was you were going to have to get people who had already done science, who had done research work. By sending our own people back to school to get Ph.D.'s, then I had the whole thing tied up with new Ph.D.'s and Ph.D.'s that we were actually giving people the opportunity to go back to school and earn. Some of the people were sent to school, got a Master's degree and then came back to the survey without getting a Ph.D., but most of them continued on until they got their Ph.D.

The problem was in the field; with all this attention being paid to new people, sending people back to school and getting a research program started on a whole lot of things that no one had ever worked on before in the division, there was a lot of resentment.

Lage: From the groundwater and surface water people?

Leopold: From the Groundwater and Surface Water Branches because they felt that the basic data program that they had all done work on all their lives was not given very much attention. So what Ray Nace and I had to do was to try to explain to people that we were perfectly cognizant of the importance of the basic data program. What we were trying to do was expand our total work. We pointed out that no research people can do anything without the basic data. The basic data is just as important as the research, because most of the research depends upon the basic data itself anyhow.

Reorganizing the Administrative Structure

Lage: Did you reorganize the structure of the division?

Leopold: Oh, yes. I immediately started the reorganization.

Lage: How did that work?

Leopold: Nobody liked that either.

Lage: People don't like change.

Leopold: No. I set up a new branch, which was called the General Hydrology Branch, where all of the research people were located.

They were not in the Surface Water Branch, and they were not in the Groundwater Branch or the quality branch, they were in the General Hydrology Branch.

Lage: Did you institute the organization by state, having a state chief?

Leopold: That was the big change that was made, and that was the part that caused so much controversy.

Each state had up to this time had three distinct offices, and they often weren't in the same building. There was the Surface Water Branch, and then someplace else there was the Groundwater Branch, someplace else there was the Quality Water Branch. Not all states had Quality Water Branch offices, but every state had a Groundwater Branch and a Surface Water Branch.

It became clear to all of us that there was no single person you could talk to. You had to talk to three people to know what we were doing. And furthermore, that did not allow you, then, to cross these branch lines. So we changed the structure and set up an organization in which there was a water resources district engineer or district officer called a district hydrologist who was the officer for the whole state.

This caused a lot of turmoil because now the people who had really pretty cushy jobs, I'll tell you--. When you got to be a district chief from the Surface Water Branch, you had a really good job. It was relatively easy. You just had to get along with cooperators, and it was a pushover.

When you started to have a district chief that had to merge these people, then the job became much more difficult, because now you had a lot of personality problems that people didn't like, and the branch representatives felt that they were more submerged. And some of the people that I chose were good, and some of the people were not good.

Lage: How did you choose them? From the ranks?

Leopold: From the ranks, yes.

Lage: Did you have a way of deciding who to choose, or evaluating?

Leopold: Between Ray Nace and Director Nolan and the branch chiefs, Joe Wells and Nelson Sayre, the chief of the Groundwater Branch, we knew an awful lot of people. Then there was Albert Fiedler, a very competent groundwater engineer who we made an assistant chief. He was a tremendous help, because he was not only

competent but very popular. So there was a lot of input from the senior officers and from the director himself. Some of the choices were good and some just didn't work out very well. But that's the way it went.

Lage: Did it involve new offices also? Moving the three branches together under this one chief?

Leopold: Yes.

Lage: This all took a lot of money too, I would think.

Leopold: Oh, yes. Now, to a great extent it was merging the offices, although it differed from one state to another. Usually there were several offices in each state anyhow. Let's take California, for example. The major district office in the California district is in Menlo Park. But then there's a sub-office in Sacramento, and there are sub-offices in several other places. In a state of this kind there would be, I don't remember how many, but maybe five subdistrict offices. But the main thing had to be run by one person. He might be a groundwater man, he might be a surface water man, he might be a quality water man. But anyhow, it would be some one man now in charge of the whole organization in the state.

Then the General Hydrology Branch, the new branch that I created, was basically overseeing the research people.

Lage: Did they also report to the district?

Leopold: No. They reported to me.

Continuing Research as Chief: A Random Walk with Walter Langbein

Leopold: Then in addition to that, I appointed Walter Langbein to be the chief scientist and gave him that title, chief scientist. Obviously, then, he didn't have to do any of the administrative work anymore, except anything that was important I would go in and talk to him, or Ray Nace and I would go talk to him and get his advice because he had been much longer in the survey than we and knew a lot of people. But he also was a very, very smart research man, and he knew problems in the water field. So he was tremendously important--the most important man in the survey.

Lage: How old a man was he at this time?

Leopold: Walter?

Lage: Yes.

Leopold: Walter--. I'm now seventy-five. Walter was about five years older than I, and he died at the age of about seventy-two, I think. He died about seven, eight years ago.

Lage: So he was five years your senior.

Leopold: Approximately, yes. But he and I were very close. We wrote many papers together. The way it worked is this: in practically all these things, including the research itself, the ideas were usually mine. As far as Walter and I, our cooperation, is concerned, he was often usually the one who could take an idea and do the mathematics, which was very important, and sort of add to it--in other words, see new ways of doing it. I'll give you an example.

We went down to La Jolla on a trip together to have a discussion about certain water problems with Roger Revelle, a famous scientist at La Jolla. Roger Revelle had called together a group of people, maybe ten people I suppose, among them one of my former professors from Harvard whose name is Harold Thomas, a professor of engineering. I had taken a course from Thomas when I was doing my Ph.D. there.

At the end of the day we were sitting, I remember, in a patio of the hotel, and I fell in conversation with Harold Thomas, whom I hadn't seen for a good many years. I said, "Harold, what are you working on these days?" He said, "I'm working on the problem of the movement of water through a medium such as sand, and I'm looking at it in terms of a random walk." I said, "That's very interesting."

About an hour later Walter and I were on the plane together, and I said, "Hey, I've got an idea. Random walks, I've never thought about it before, but that has got something to do with us." Now, I said, "First, random walk can be thought of as how a drainage basin develops by chance. Random walk can be used for the movement of stream channels. Random walk might be used in several kinds of groundwater problems. It could also be used in hydraulic problems." I said, "Now, we have to exploit this."

Well, that's all you needed, one word, and all of a sudden Walter and I started to churn out ways in which random walks could be used. Now random walks are very, very common.

Lage: It's a common concept? What exactly does random walk mean? Does it mean that things develop by chance?

Leopold: If you have two rills on a hill slope that happen to be starting down the hill slope by chance, and they're a certain distance apart, and you can ask yourself, "What is the chance of those two streams meeting and becoming one?" If you take each one and assign it a random chance of moving right or left and then you follow them down, each assigning by chance whether they turn right or left, how long will they go before they meet, or will they meet?

When I posed that problem to Walter, he came back in a little while and he said, "That can be expressed by the statistical problem called the Gambler's Ruin. I said, "How's that?" "Well," he said, "the way it works is this." He said, "Your chances of winning at Reno depend on the relative capital that you have against the house. Since most gamblers can never have the capital that a big casino can have, they simply can't win in the long run. You can describe this by the chances of two things coming together, and depending upon, in this case of the Gambler's Ruin, the question of who has the capital behind him. And here is the equation that describes the gambling thing and also describes the question of how these things meet." It turns out that that was the equation that was needed. He supplied the equation, I supplied the idea.

Lage: Did you apply it to the various problems you mentioned?

Leopold: Oh, yes. One of the lectures in the courses that I give in the summer is based on this. I got interested in the problem of the branching of streams, and we had published this paper on the random walk, and we showed for the first time that the joining of streams actually was a random problem, a completely random problem. I said, "Therefore, if you have streams that join in a random manner, how about the branching of trees?"

You're changing now from a two-dimensional case to a three-dimensional case. We had hired on our staff on a temporary basis the great geophysicist, Adrian Scheidegger, a very famous guy who was so theoretical that nobody could understand him.

Lage: How did you happen to hire him?

Leopold: He was at the University of Illinois, I think. He was so smart that Langbein and I decided that he was somebody we needed around. He wrote a book on theoretical geomorphology that nobody could read, it was so complicated. I can't read it.

Lage: Did he write it as a result of his time with you, or was he already working in geomorphology?

Leopold: I think he did it because he was associated with us, because we got him interested in it.

I turned to Scheidegger and I said, "Adrian, Walter's worked out the mathematics of a two-dimensional form of river channels, but I want you to see whether you can derive a theoretical analysis of a three-dimensional thing like a tree." He worked on it for weeks, I guess, and said, "No, it's impossible. I just can't do it." And I considered him one of the great mathematicians of his day. I said, "Okay, if you don't do it, then I will."

So my chief administrative officer came into the office one day, and he said, "Chief, do I understand that you asked me to order two boxes of Tinker Toys?" I said, "Yes." "What in the world do you want Tinker Toys for?" I said, "I'm going to build a three-dimensional tree." So I took the Tinker Toys (you remember what they are) and you'd toss a card, and the card would tell you, do you add a stick or a round wheel, how many do you add, and what length. So by tossing cards I built up this tree, a three-dimensional tree, out of Tinker Toys, but for each one, the decision to do something depended upon the toss of the card.

Then when I analyzed the tree, it had the same characteristics as the rivers did. So I published this paper called "Rivers and Trees, the Efficiency of Branching Patterns." Then I showed the difference.

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Leopold: Then I went to real trees and showed that the analyses that you can make of river channels could also be applied to trees. You got the same result whether you were dealing with the random Tinker Toy tree or a real tree.

Lage: So the pattern of the random Tinker Toy tree was the same as the pattern observed in real trees?

Leopold: That's right. Yes. Now, I had no theory to--. I simply showed that this was true.

Lage: You didn't have the mathematics.

Leopold: There is no mathematics that was available to do it. I simply said, "This is the way it is." But then I said, "Well--." Remember, now, as chief I spent at least three months of the year

on my own research. I simply let somebody else run the organization. So I said, "There must be, as in rivers, there must be some efficiency to be gained by this; the random pattern must develop into some kind of efficiency."

In my back yard I was growing a sunflower plant about this high [1 meter]. The sunflower plant had rather large leaves, so I took this plant and I painted a big number on every leaf. Then I wanted to know what--. You remember how beautifully organized trees--if you took a mesquite plant, how beautifully organized they are. So I said, "They must be arranged in such a way that they get their sunlight in some efficient manner."

So I took a light camera and I put it in the end of a fishing pole, and over the sunflower plant I took photographs of different positions of the sun as if the sun were looking down at the plant. Then I took the photographs and with a planimeter measured the amount of surface of each leaf that would be seen by the sun and added them over the passage of the sun. This gave me the number of square inch-hours obtaining direct sunlight during a whole day.

Then I said, "All right. Now I'll compare that with a theoretical plant which is shaped like a hemisphere having exactly the same total leaf area as the real plant. Now I'm going to do the same thing, and I'll pass with the same angles over this hemisphere and find out how many square-inch-hours of direct sunlight it got. And I showed that the actual plant having exactly the same area as this dome was more efficient by 20 percent."

So then I said one of the reasons that these random patterns develop is for efficiency.

Lage: Why would you expect that the random development would be more efficient?

Leopold: Because, two things. First, the utilization of energy in natural systems in practically all cases moves toward the most efficient use of that energy. This is described, at least to some extent, in the theory of entropy.

But entropy also involves the question of randomness. For example, the one example that we used was: when you have your desk cluttered with material, it becomes random, and you don't have anything organized, because if you pick up a sheet of paper here, that may or may not be the one that you're looking for. But when you start organizing them in the form of files, you are decreasing the entropy by putting energy into it. But by

organizing it into slots, you are decreasing the entropy, so that the more organized it is, the lower the entropy. The more disorganized it is, the higher the entropy. Therefore the efficiency of how your files are kept is proportional to the work, or energy, put into the system. That's really one of the aspects of entropy.

So that efficiency and maximum probability go hand in hand. I was trying to show that the way in which natural systems are organized is partly random and partly for efficiency. This was just one example.

Lage: I know this is off the subject, but it's fascinating. Do botanists look at it in the same way? Or did you talk to botanists about it?

Leopold: My brother is a very well-known botanist. He's a professor of plant physiology. I sent it to him, and I said, "Have you ever thought of this?" He said, "No. It's an interesting idea, but no botanist has ever played with it before."

I sent it to Ecology, and they turned it down. I sent it to another scientific journal and they turned it down. It sat in my file for several years. Finally a man came to see me. We were talking about this problem, and he said, "I know the editor of a journal called The Journal of Theoretical Biology. Why don't you send it there?" They were delighted; they published it. Theoretical biology. Well, anyhow, that's an example.

Many of the things that I worked on--and a lot of them were together with Walter Langbein--were both complicated and not very well received.

Lage: By--?

Leopold: By scientists in general. Like a lot of things that go on in science, things can be simply sitting on the shelf for years without anybody picking them up, especially if they're complicated. In other words, it's a lot easier to take some relatively simple picture.

A lot of things we worked on, especially my work with Walter Langbein, have not really been followed up by people because they are complicated, they are different. And all this was going on when I was chief, you see. In other words, I always had something on my desk that I was working on in my own research.

One of the things that I did, for example, was to say to my senior officers in the Washington office, "I want each of you to

take at least a month, preferably two months in a year and go away. Any time that you want to, walk out of the office and go someplace and do something else. I don't care what you do. Go to the district office and help them with administration, or collect data, or go out and measure streams, or measure wells, I don't care. But get out of the office and go get some new ideas."

No one in the whole organization did that except I. I would simply walk out in the summer and say, "Okay, you guys run it; I'm going out to do my own work."

Lage: Why did they not get out? It seems like you would pick the kind of people--

Leopold: Because most of the people who had gotten up in higher grades had not followed research for a long time and they had lost touch.

Lage: They almost didn't know how to do it?

Leopold: They didn't know how, and they were afraid to go back to data collection. They were either too old to get help measuring streams, or--. There are a lot of reasons why they didn't. But the point being that the difference was that the organization was being run by somebody who was on the research team himself, and that made a lot of difference. And when I left, you see, the whole thing fell apart because they didn't put back in that job people that were actually research people themselves.

So this was simply a hiatus in the history of the organization, and it's pretty much gone back to where it was before, except that now research is spread widely through the organization. But the top people are not research people.

Lage: But the research program, it seems, is still ongoing.

Leopold: The programs kept going. But the idea of somebody himself doing research at the top level has simply not been duplicated.

Independence for Researchers

Lage: Earlier you were talking about hiring the young scientists and letting them finish their Ph.D. work, and then you said, "After that, we'll talk about your program." After that, how did you work with them on what they would research?

Leopold: All right. I would call a man in to talk to me, and I'd say, "What I expect of you is this--" and you can imagine how much this has changed. I said, "I want once a year a one-page statement to tell me what you are doing and what you intend to do. That's all I'm asking." That was it.

Lage: What if you didn't like their one-page statement? What if they weren't significant problems or didn't fit the goals of the organization?

Leopold: Then there are lots of ways to urge and encourage and that sort of thing, but I did not tell people what to do.

Lage: You didn't tell them what to research?

Leopold: No. I said, "You know the field better than I. You pick out what you think are important problems, and you do it your own way." And then further, I said to them, "I'm going to give each of you--" this is the research people-- "I'm going to give each of you an amount of money equal to your salary to do with whatever you like. You can have a secretary, or you can go to the field, you can spend it for travel, you can spend it on your office, you can spend it any way you want to. Beyond that, you then have to compete with everybody else. If you want a big laboratory, then you have to wait. We'll program that."

For example, some people said, "I need a chemical laboratory." "Fine," I said. "Okay. Your turn will come up such-and-such a year. You will have to wait, but the time will come up, then we'll built this whole damn laboratory from scratch and set it up the way you want it." So that everybody had enough money to do something.

Lage: Did this system work, do you feel?

Leopold: Oh, it worked extremely well. Oh, yes, you bet. Oh, everybody loved it. Because in the first place, they were doing what they wanted to do. Within about six years after I was chief, the papers that were coming out by the Geological Survey were the most famous papers ever written in the field of water, on every kind of subject. No question about it; this was the place to look for the current thinking. And of course, that's all gone to hell too.

Raising Expectations in Publications and Hiring

Lage: You did something with publications, too.

Leopold: Oh, yes, you bet. First--and this was not entirely my idea, but I was the one that was pushing it--we had always worried about the fact that it took such a long time for the surface water records to be published, because they were published in the Water Supply Papers by river basins. Most of us felt that one office was slow, one office was fast, and you had to wait four or five years before you could get hold of the data. So we decided we were going to split it up by states. So the district chief of that state would get out his report as soon as he could.

Well, this has been tremendously successful. And then, of course, everybody was competing with the other guy to see who'd get it out first. So instead of waiting five to six years on the average, they were getting their reports out in about six months.

Lage: What happened when the river basin cut across several states?

Leopold: No. The state still took all the stations that it was responsible for.

Lage: You have a book there.

Leopold: Yes, here's one. [gets book] Now, here, the new ones have quality water, sediment, and surface water, whereas the older ones, you see, had nothing but surface water. So that the gradual change that was made by the integration of the offices allowed us to publish on a rather quick basis all the aspects of water that were being studied. So that was a big improvement.

Another thing that came up through the branches as a result of encouragement of research was this. Under the leadership of Joe Wells when he was the chief of the Surface Water Branch, we wanted to develop a method by which you could somehow scan the ink trace that we'd get from the water surface chart and turn it into numbers. So they were looking at essentially a scanner which nowadays would be relatively easy to do.

But at that time, they spent a lot of money on it and it wasn't working out terribly well, and one of their chief scientists in the Surface Water Branch, whose name was Rolland Carter, he said, "Let's try it a different way. Let's see whether we can put it punched on a tape." Well, now, this is the way it's done. He worked with one of the manufacturing companies to replace the inkline with a punch, basically like the computers

now. Now what they can do is take the punched tape and read it like you read the yes and no answers on an examination. You can read the tape and turn that into a computer. And now everything's done by computer. So anyhow, a lot of things were going on.

Lage: A first step towards automation.

Leopold: Automating the whole system. That came out of the encouragement of the research activities in the branches. It was Carter's idea--

Lage: Was Carter a research person then? Not a surface water person.

Leopold: He was both. I don't know exactly how he was described, but I would call him one of their chief researchers. At least I thought that he was a chief researcher. And I think he was paid as a researcher.

Lage: Were these kinds of changes accepted well, or was there resistance?

Leopold: Yes. It took the district quite a long while to get used to putting the data out by states, but as soon as they saw the possibilities, then they became very enthusiastic about it.

And then there was another very important thing that we did. We found, you see, that we didn't have the right kind of people coming to work for us. Now, this took two forms.

In the first place, in addition to the district hydrologist who was in charge of a state, we divided the whole country into regions. One of our regions was in St. Louis, and it was run by the young surface water man whose name was Wilson. We'd had a long discussion about how to get good people to come to work for us, and I said to Harry Wilson, "What we ought to do," I said, "is to not hire anybody but Ph.D.'s."

He went through the ceiling: "That's impossible. For goodness sakes, right now we can't even get the lowest engineer. The poorest grades, they won't come to work for us." I said, "The problem is that you're not setting your standards high enough. I'm convinced that if you said, 'We won't take anything but the very best,' that you will improve the position of hiring." "No," he said, "it absolutely won't work. My God, we can't even get the lowest one on the totem pole."

I was passing through St. Louis, and I had a conversation with Wilson, and I said, "Have you thought about my plan?" He

said, "Well, I'll tell you. Why don't we do this? I'll agree to use your plan for six months, and if it works, we'll do it. But if it doesn't work, I want to prove to you that it simply won't go." I said, "All right, Harry, you do it. I'll give you six months. You follow completely my plan."

So they went to the schools in that state or in the states around, and they were interviewing people and said, "We're not even interested unless you're in the top 10 percent of the class." All of a sudden, by God, we started to have people that were applying all over the place. Nothing but the best. I said, "Okay, you see?" So now the thing changed entirely. After that, the survey wouldn't even talk to college students unless you were in the very top of a class, whether you would be a chemist or an engineer or a geologist. Well, it changed everything.

Lage: So you had a convert, I would guess, after that six months.

Leopold: Oh, yes. He said, "I wouldn't have believed it. It really worked." So anyhow, then the whole business of hiring changed.

Lage: Were this hiring for the research program or for data collection?

Leopold: For both. Usually-- Well, you see, we needed engineers to run the district programs too. And we needed geologists to run the district programs. So we wanted people that were the best people that were being trained.

Promoting Education in Hydrology in the Universities

Leopold: In addition to that, Langbein and I decided we had to start a school. We weren't getting the people that we wanted because either they were engineers or they were chemists or they were geologists, but nobody was across the field where you were a hydrologist.

One of our people that we admired a lot was John Harshbarger who was a geologist in Tucson. He was interested in the kinds of things that we were doing, so Walter and I talked to John and said, "How about starting a school for hydrologists under you?" I said, "We will furnish the teachers, and you make arrangements with the university." John had very close relationships with the university.

Lage: Was John with the survey?

Leopold: Yes. He was a groundwater man with us, and he was the kind of man who was very, very good at dealing with people and persuading them and that sort of thing. So it was decided that we would set up a school for hydrologists in the University of Arizona at Tucson. John would be a teacher, and we sent three other people to be teachers.

Herb Skibitzke, my friend with whom I flew all the time, lived in Phoenix. Herb used to take off from Phoenix in his airplane and write his lecture notes on the airplane while it took three-quarters of an hour to fly to Tucson. He'd leave his airplane, go to school, teach, fly back to Phoenix.

Lage: He'd be flying himself as a pilot and writing his lecture notes? [laughs]

Leopold: Yes. He was a marvelous guy. Well, it was very successful.

Lage: What level of training was this?

Leopold: In the graduate school. I think that the credits were applicable in the graduate school, but I suspect that some of the students that came to take those courses were also undergraduate. But they could get graduate credit if they wanted to.

Lage: So they got this kind of synthetic approach.

Leopold: Yes. But you see, we were teaching groundwater, surface water, and water quality altogether. Herb Skibitzke was teaching groundwater theory. Harshbarger was teaching about groundwater practice in the field. One of the men was teaching the mathematics of what they called systems analysis. The mathematics of systems analysis applied to water resources work. We had one man who was a groundwater theoretician. I don't remember who was teaching the surface water part.

But once we started to turn out people now who were trained across the board in hydrology, then everybody could see that this was going to be very successful and other schools started to do the same thing. So within two or three years there were hydrology courses being taught practically everywhere. Everybody just jumped on the bandwagon. Other schools could see that this was a field where there was opportunity to be hired, it was a broad scientific field of inquiry, and so now we have hydrology being taught in at least dozens of universities throughout the country. That was the first time that hydrology was taught as a field.

Redrawing Civil Service Requirements for Hydrologists

Leopold: At this time, we weren't satisfied with the kinds of people we were getting, and the civil service began to realize that they were too narrow. So the civil service called me in and said, "We would like to have a new program of a civil servant called hydrologist." I said, "Fine. I'll write it for you."

So I personally wrote the civil service requirements for hydrology patterned on my own experience at Wisconsin, where I was in the engineering school, but I took plant physiology, geology, botany, ecology, taxonomy. Then various levels of geology, including paleontology. So in my own experience I could see how this could be done. To be a hydrologist, the civil service requirements that I wrote included a certain amount of work at the college level in chemistry, physics, mathematics, biology, geology. That was basically it.

Lage: Did you write those before you got these schools stimulated?

Leopold: It must have been about the same time. That's what we were teaching in the university. So we were teaching people who fitted into the field of hydrology.

Now, to show you how different this is and how it's been degraded: in connection with this big law case that I've been involved in for the Forest Service, it became quite clear that the Forest Service was very short of hydrologists that knew the field. Although they have a big research organization, the research organization is so disparate from the operational part of the Forest Service that the Forest Service operations had no hydrologic help.

It was decided by one of the assistant chiefs of the Forest Service that they were going to finance some work by one of my students. When I heard about this, I went to the Forest Service and said, "That's not the way to spend your money. You can put a lot of money into one person, but that's not what you need to do. What we need to do is to train people already in your organization on a much wider basis." I said, "Use the same amount of money, and you hire me and my friend Dave Rosgen," who used to be in the Forest Service--he is a very competent hydrologist-- "and we will teach your people."

So last summer Rosgen and I gave two courses, thirty students each. Each course lasted a week. We took them to Pagosa Springs, Colorado. They were all Forest Service personnel.

Lage: With different backgrounds?

Leopold: Yes, various backgrounds. Most of them had some possible smattering of hydrologic background, but usually not. Quite a few of them were called hydrologists.

At the end of one of the two courses, I was talking to a young person who was the hydrologist for one of the forests in Alaska. She had been quite quiet during the course. The course was over and they were all about ready to leave, and I happened to fall in conversation with her. I said, "Tell me, how did you get to be a hydrologist in the United States Forest Service?" She said, "I have a degree in environmental science." A bachelor's degree. I said, "That's interesting. How much hydrology did you have?" She said, "I didn't even have a course in hydrology." I said, "You're called a hydrologist and you've never had a course in hydrology?" "No," she said, "everything you talked about was brand new to me."

Lage: And yet civil service hired her as a hydrologist.

Leopold: But what I'm saying is, you see the difference. In other words, we were saying, "We know what a hydrologist has to know." You can't teach him everything, but he's got to have a background that involves a whole lot of things, including hydrology.

Lage: And these requirements weren't just used to be hired by your organization, but by other--

Leopold: No, it's for the whole government. All of the government agencies. But over the years the requirements I wrote have been changed and diluted.

Revising Publications Policies: The Pink Terror Memos

Leopold: So anyhow, you can see that a lot of things were going on at that time. Training, the expansion of the field of inquiry in the whole field of water, writing, and publication. The survey has always been very proud of its publication program, and indeed the publications have always been extremely good and very carefully reviewed.

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Leopold: But publication has also been very slow.

Lage: These are the research publications we're talking about, rather than the data?

Leopold: Both. Yes. Mostly the research publications.

Two interesting aspects of this. One of the young people that I hired was writing a report on a certain geomorphic problem in California. He was one of the people we sent to school. He had taken his degree at Harvard but was relatively new both in the survey and the research team. He wrote me a long letter in which he complained that the more senior people in the organization were basically directing his research. They didn't like his ideas, and he felt that he was being prevented from being innovative and fresh.

So I wrote a letter to him, without his name on it. Such policy memoranda came to be known as the "Pink Terrors."

Lage: The Pink Terror memo?

Leopold: Yes. They were published on pink paper. This one said--

Lage: Oh, there was more than one?

Leopold: Oh, yes, there were quite a few. This one said, "It has long been supposed in the Geological Survey that what the survey agrees to publish is true." I said, "In science, that cannot be guaranteed. My policy in this division is as follows: I will guarantee you that I will publish anything you write, no matter how different than the usual thinking, provided that you have a copy read by somebody in or out of the survey who will give you comments." This was a kind of peer review, but I think I asked for two people to read it. I said, "All I want you to do is to write a memorandum which tells me what were their criticisms and what you did about it, but you do not have to follow their advice. You merely have to pay attention. With that understanding, we will publish whatever you write."

Well, this made a lot of difference. Because now people were feeling they could write what they wanted to, what they believed to be true. But I said, "We cannot guarantee the truth of what you say. We'll only guarantee your right to say it."

Lage: Previous to that, the publications of the survey--

Leopold: It was always supposed, you see, that unless something could be tacked down to be absolutely right, it wasn't going to be published by the survey.

Lage: It couldn't be very theoretical.

Leopold: It prevented people from expanding into new ideas. So I sent a copy of this to the chief geologist--the division chief comparable to my own position. I said, "Here's my policy. I wonder if you will publish the same thing, or something like it." He turned me down. We were the only ones who said, "We want any ideas that you've got, providing it's well thought out, carefully presented."

Lage: Did you run into any trouble with that? Did you get any unfortunate research?

Leopold: No, no, no, everybody loved it.

Lage: But did you get any unfortunate papers as a result?

Leopold: Yes. I remember one guy we sent to school. He had really gotten right up to the end of the Ph.D. program. At that time, I was reading, myself, every single paper that anybody prepared for publication. In other words, when they came through for our permission, I read it. I read it. Now it's not done that way at all, anymore. I saw this manuscript, and I called this guy in, and I said, "In the mapping of this geology, you said that this particular feature was a kame terrace."

Now, a kame terrace, if you possibly remember, is the collection of material on a hill slope up against a glacier. When the glacier melts, the terrace is simply a little sort of a hanging deposit on the side of the hill. I said to the young man, "How did you know this was a kame terrace?" He said, "I don't remember." I said, "Why don't you get your notes out?" It turns out he didn't have any notes. I said, "I won't publish this. That's not the kind of science that we do." There's an example.

Lage: So you had a way of checking on quality.

Leopold: The thing is, in this particular case not only was he supposed to have somebody else read it, but I picked up this thing and I said, "I want to know how you did this," because it was in a situation where I didn't think that this could be seen, and I wanted to know how the heck he'd made this statement. This is what peer review is supposed to pick up. So yes, we had some

cases where people were turned down because they didn't prove to their compatriots that this was a well-reasoned argument.

Encouraging the Flow of Ideas

Leopold: Then there was another aspect of it. One of them was that I wanted somebody on our staff at all times, some person from a foreign country. So I set out to bring people over to work for us for a year and let them do whatever they want. So that somebody from another country got acquainted with what we did, and our people got to know how other people thought.

Well, that was very successful in certain cases and less successful in others, depending upon the type of person that I actually chose.

Lage: Would you have some way of introducing them? Did they give workshops or seminars?

Leopold: Usually they picked a problem where they wanted to go to various parts of the country. So they got to know people in various parts of the country.

Then I was very concerned about presentation. In the Washington office, whenever somebody arrived in town that I thought had interesting things to say, I could immediately pick up the telephone and within a half an hour have a dozen research people in the Washington office get to my office and hear this guy give a seminar. So I was very concerned about the constant flow of ideas, particularly when somebody came through town that we didn't know.

But then I was also concerned about how people learned to speak. I'm still laughing about this one. The Surface Water Branch was holding a big seminar, people from all over the United States. Many of these seminars or discussions that the branches had, I would go and listen to myself. Well, I went to listen to this one. A man got up and started to talk, and he started to show some slides. They were so bad that in the first place, nobody could read them, and nobody could understand what he was saying.

Right in the middle of this crowd of a hundred people or so, I stood up and said, "Stop." I said, "I won't stand for it. I'm paying you people, and by George, if you're going to come here and take up the time of a hundred people, you're going to be

better prepared and you're going to speak clearly, and you're going to have slides that we can read. Would you please take a piece of chalk in your hand and go to that blackboard, and you tell us in words that we can understand what you have to say." I'll tell you, it had never happened before. But God, you could hear a pin drop.

Lage: Did that kind of thing have repercussions? I think that would make them just shudder, the thought of your walking into one of these seminars.

Leopold: But everybody started to pay attention. But boy, that had a hell of an impact. It had never been done before.

Lage: What were some of the other Pink Terror memos?

Leopold: I don't remember, but somebody told me a week ago, somebody told me that one of the Pink Terrors had been reproduced and was now passing through the survey offices here in the western United States last week. He said, "The same thing. Just what you wrote. It's so applicable to what's going on."

Lage: [laughs] Fascinating.

Leopold: I don't remember what they were about.

Retrospective Views on Leopold's Changes in Program and Management

Leopold: The thing that people talk about now in the division is how they disliked and distrusted what we were doing. Looking back at it, they said it's the best damn thing that ever happened to us. They realize now that we were on the right track, but they didn't like it at the time. That was really the key to what was going on.

Lage: So you've had that kind of feedback since you left?

Leopold: Oh, yes. And they're still talking about it. Those were the days when things were really going on. And to everybody's advantage. The thing that is amazing is why this kind of management did not persist. What we did structurally, the importance of the research program, that has expanded greatly, but the management view has not persisted.

I'll give you one example of this. I hired one biologist--we'd never had a biologist before--to start a program of biology and water. There are now forty research people in the field of biology, in the division. You can see what happened. Once you got it started, then all of a sudden they see, "Gee, biology is very important in the field of water." It's involved in water quality, involved in the flora and fauna of streams, in hydraulics--the effect on roughness--and particularly in chemistry. Anyhow, these things are ongoing, but, as I say, the management style has not persisted.

Lage: But when you say management style, I think of sort of the technique of management.

Leopold: No, that's not what I mean. The only thing you can say about technique is this. Differing from anything that went on before, when I went to the field office, I never talked money. The district chiefs used to get so angry at me because I'd say hello to them, then I'd walk down the hall with all these people, you see, in a big room doing something. I would sit down with somebody I'd never met before and I'd say, "Hi, I'm Luna Leopold. What do you do?" "I do this." "Tell me about your work." Instead of talking about budget, my idea was I wanted to know what the people do, what they think about, what's important to them. Those were things that I think were--that's what I call style.

Lage: So you worked with rank and file too.

Leopold: I tried desperately to do so.

Lage: Did you keep in touch?

Leopold: I had 360 offices, and I visited a large number. Certainly not all, but I visited offices overseas as well as in most states.

Lage: If you didn't like what this rank-and-file person told you, was there--

Leopold: Then I never criticized him. I would say, "Here's a suggestion. You might consider this." And then I'd talk to the boss, you see. But it never was critical. Rather, the thing is that I had a lot of ideas about how things might be done. They usually felt that they got kind of a lift. They said, "Gee, here's somebody

thinking about my problem and giving me suggestions of how I could do this better."

Lage: It makes his work seem significant also.

Leopold: Yes.

VII EXPANDED SCIENTIFIC PROGRAMS IN THE WATER RESOURCES DIVISION

[Interview 5: February 6, 1991]##

Changes in Data Collection: Network Design, Benchmark Gauging Stations, the Vigil Network

Lage: Today is February 6, 1991, and this is the fifth interview with Luna Leopold. We're in the midst of talking about the Geological Survey and your experiences in managing the Water Resources Division. I had a few questions about the data collection program. You have mentioned that you made an attempt to expand data collection, but I think the implication was it wasn't as successful as you had hoped. Were you trying to get new kinds of data?

Leopold: Yes. Let me explain a couple of things about that. I forgot exactly how many gauging stations we were running at that time. It was about 11,000, I believe. It was quite clear that the original idea that had been long held by engineers in the Water Resources Division, with regard to the longevity of the gauging station, was not going to work, in that you could not afford to keep all gauging stations going indefinitely. The question came up, what are we going to do about this? You can't, in other words, have gauging stations running forever and still increasing the number without limit.

About the middle 1950s, Walter Langbein published a paper anonymously in which he said, "How long should we run a gauging station?" Well, this caused quite a stir. In the first place, nobody was supposed to know who it was. Later on it became clear who wrote it, but he was saying, in effect, "We're going to have some sort of a scheme to determine how long a gauging station should be run."

Remember, now, that there are two kinds of gauging stations. A small number were financed completely out of federal funds. Most of them were financed cooperatively by having the state pay 50 percent. In the latter type of gauge, the location was usually chosen by the state. In other words, the state needed it for water management purposes or for the distribution of water or for measurement, and they simply put up half the money, and the Geological Survey ran the station for them.

What Langbein had suggested was that we basically should have two types of stations. One would be called a water management station and the other would be basically a base station, if you like. The base stations were to be chosen to be representative of various parts of the country, and they were to be run more or less indefinitely. The water management stations were to run for a short period of time and then be discontinued. But before they were discontinued, the idea was that their records would be compared with one of the base stations until a correlation could be developed so that you could make an estimate on the basis of correlation. If the base station had a certain discharge in a particular year, by correlation you could estimate what the coordinate or the simultaneous discharge was at the discontinued station. This actually did become the procedure that we adopted.

Lage: Did this mean disruption in people's jobs?

Leopold: No, no, it was simply that it was so different than what everybody had assumed, that the longer the record, the better the record was going to be.

Now, what we finally decided on was that there were to be stations that were to be run indefinitely, but all these stations, of course, were subject to not only the changes of climate but also to the changes caused by man. Therefore I devised a scheme that we have another set of stations which I would call the benchmark gauging stations. I started out with a small number; I think there were about ten about that time. We asked the district offices to find places where there would be a stream that for one reason or another would be indefinitely protected against man's incursions, such as in a national park, in a national monument, in certain kinds of other protected areas.

We would install a gauging station at these selected points, and they were to be really the long-term stations that represent the natural condition unchanged by anthropogenic effects. We visualized that the first type of measurement which was to be made would be ordinarily the same kind of measurement of water

discharge that would be made at an ordinary gauging station, but the idea would be that these stations would later on have chemical, biologic, and other water quality parameters in addition to the flow rate.

Lage: That was something--

Leopold: That was brand new. I called those the benchmark gauging stations.

Well, this has been now, let's see, it's been forty-odd years since the first benchmark gauging station was run. They've turned out to be so successful that the Water Resources Division has gradually expanded this program, and now we have, I understand, something in the order of forty of these. They are considered to be one of the best things that the division's ever done.

Lage: So that's something that continued after you left.

Leopold: Yes. That's one of the things that did continue. As a matter of fact, it expanded.

The other thing that I started was another system of data collection which I ended up calling the Vigil Network. Have I spoken about that?

Lage: No, not at all.

Leopold: It started this way. When I was in New Mexico in the 1930s, we were trying to determine the rate at which the great trenches or gullies were enlarging through time, which started with the climatic change in the last century. We knew that Professor Kirk Bryan from Harvard had surveyed several cross-sections on the Rio Pureco, one of the great gullies of the world. We wanted to re-run those surveys that he made in order to find out what had happened since his survey was made in the 1920s.

Well, there were no notes and nobody could find the cross-sections. So I said, "Since we can't find them, let us start over again, and we will put in some cross-sections that would be well monumented and would be carefully recorded so that twenty years from that time we could re-run our own cross-sections."

I came back after the war and found that when I tried to send somebody out to locate these stations, the notes had been lost. Then I wanted to look back at the measurements that I had made on the Navajo reservation in 1933, where I had been employed by the Soil Erosion Service to actually map the vegetation on

certain fenced plots. Those were supposed to be long-term measurements of what would happen if you didn't graze in that kind of an area.

I went to the Department of Agriculture and said, "I'd like to see the maps that I made in 1933 on the experiment station," and all the maps were lost. So here are two times in my life the things that I knew were very important had actually been lost. I said, "Let's then start something brand new. Let's have a procedure by which the original notes from surveys of this kind will be stored in two places forever: the Library of the United States Geological Survey in Washington, and the Laboratory of Geomorphology in Uppsala, Sweden. Duplicate copies therefore will be available, so that a hundred years from now somebody could go back and redo what we had done many years before." I presented this at an international conference in Italy, and the Italian words that I used to describe the system created the acronym "vigil," which means "watching."

Well, now this has become extremely important. I think there's something like two hundred-odd such surveys now in the Vigil Network file. Just this year, in 1990, two of my friends in the Geological Survey in Denver are now trying to advertise, if you like, to get more people to contribute to it. So there is a paper now in press which is calling attention to people throughout the world that there is something that is vitally needed for science, and if they will make the survey according to the descriptions that we have written and published, they would be on file forever in two places.

Lage: So there's a description of how the data should be collected and what kind of data.

Leopold: Yes. It's very simple. Very simple. In other words, this is not something very fancy. The point is, if you put two iron stakes in the ground and survey between them so that you get the cross-section of the channel, if you didn't do anything else, that would be useful. You'd come back and say, "How much has the channel changed over a period of time?" It has nothing to do with why the channel changed; it simply said what did happen.

So that although Ray Nace and I were criticized for paying too little attention to the data collection program, at least in the eyes of many of the older people in the survey, indeed, we had gone much farther than they had done and tried to expand the theory or the policy of how gauging stations should be done. This was one of the great contributions that Walter Langbein made where he mathematically attempted to determine how long stations

should be run under certain circumstances. It was called network design in those days.

Lage: So that was a mathematical treatment, how long the stations should be run, to see when they correlate?

Leopold: Yes, because if it was a question of correlation, you see. But his mathematics had to do with what is the most efficient combination between length and dispersal of the station locations. In connection with the change in the method of publication of the water surface records, that became an important matter.

And also, another thing that had not been done before: I felt that sooner or later we were going to have to get into the biological aspects of water. So I hired a small number, I think one or two, limnologists.

Lage: Limnologists?

Leopold: Limnologists, people who are interested in the biologic aspects, the creatures and the plants that grow in the streams.

This has been so successful that now the biologic part of the Water Resources Division is probably the largest part of this whole research effort. They have something like forty scientists working on nothing but stream biology. By stream biology I mean also the chemistry, and part of their job is to deal with the most complicated and esoteric aspects of water chemistry.

Lage: Is this because of the interest in water pollution, water quality?

Leopold: Yes, not only pollution but the whole question of all the aspects of water quality, which include the use of water for human consumption as well as heavy metals, poisonous substances, and especially petroleum products that get into water, such as phenols and things like that.

So during that period of time between 1956 and 1966, there were important changes in the data collection program. So that the people in the field may not have seen the extent to which we were actually expanding and making more useful the data collection program. But because it was different than what they were used to, it came in for considerable criticism.

Backyard Research: Strawberry Creek. Hawaiian Dew

Lage: The program of backyard research, what was that?

Leopold: That was another aspect of something I invented. Backyard research is well exemplified by my and my students' work on Strawberry Creek [on the UC Berkeley campus]. It is my opinion that a lot of very useful scientific work can be done with practically no money and using very simple procedures if people take the trouble to decide what should be measured. So when I came to this university, I installed a staff gauge out here behind Haviland Hall. When it rained, I went out there, put a rain gauge out on the lawn, and I sat there and watched the water rise on my little gauge. The gauge was nothing but a meter stick, you see, stuck in the ground. So that by measuring the depth of water every two minutes and the measurement of the rainfall every five minutes, then I could put them together and figure out what was the effect of man's use in Strawberry Creek. How would it compare with a similar watershed that was not urbanized?

That went on from 1972 until 1990. This year I have in press right now, in a scientific publication published in Germany, the report of my ten years of work on Strawberry Creek and the other creeks around here. I call this backyard research because it costs nothing. You can get a rain gauge for two dollars. You can make a staff gauge, and many of my students did, out of a stick. You simply put it in the ground. And you use your wristwatch. So it takes nothing except time and thought. I said that this kind of research could be very useful, and I tried to encourage people in the survey to do this kind of work.

Lage: People who were out in the field?

Leopold: People could do it at home or do it out in the field--observe something. I said, really, you can do this in your backyard, and it was called backyard research.

Lage: How did you communicate these ideas to people? Were they published?

Leopold: No, these were memoranda. Furthermore, then I could give examples of what I was doing. For example, I lived in Hawaii for five years. Every morning at breakfast time I went out and measured my rain gauge and read the temperature and the wind anemometer, but in addition, I made an estimate of how much dew there was on my feet. So I had five years of record, every day,

of how much dew collected on my lawn, that was very qualitative. So when I got to Washington after five years in Hawaii, I wrote a paper about this which I called "Dew as a Source of Plant Moisture." It turned out that this was the first paper of something that later on grabbed the interest of scientists throughout the world, and all of a sudden the thing expanded and lots of people were working on dew.

When my paper was done, I went to Walter Langbein and asked him to read it. He really berated me and said, "You did it just by looking at your shoes? You should have had more quantitative measurement." I said, "Walter, that's what I did. This is my backyard research."

Lage: But the ideas were there.

Leopold: The idea, and the point is that when I made a statistical analysis of my results, they were qualitative. I could turn those estimates into numbers that came out to be very good.

Lage: How did you measure it by your shoes? How wet they got?

Leopold: I said light, medium, heavy, or none. But then when I had enough data, then I could go back to other kinds of records, you see. For example, we found that in many kinds of plants like a pineapple plant, the leaves are shaped such that when dewfall forms on the leaf, the dew slides down the leaf right to the root and becomes extremely important in providing moisture to the roots. Well, this is true of all desert plants. This was the first paper, to my knowledge, that brought out the fact that in the desert, dew is very important, and here's an idea of how often dew occurs and how much water was involved. So that's an example of what I mean by backyard research.

Lage: And how did it work out in the survey? Did people take this up?

Leopold: A few.

Attempt to Stimulate Publication of Hydrology Series

Leopold: Well, then I had another idea that didn't pan out as well as I thought. We now had, after five to ten years of research, we now had people that were probably the most expert people in the world on glaciers, on water quality, on groundwater, on water levels, on discharge. I thought, why don't we write a series of books to fill a whole shelf of the library, in which there would be a book

on each one of these subjects on which we have now become really expert? The problem of climate and its effect on water supply; glaciers; river channels; geomorphology in general; on and on and on. I said, now, if this is really going to be successful, this series of books should not be published by the government.

So I went to a close friend of mine, a very important publisher by the name of William H. Freeman, who was the president of W. H. Freeman Company and who had a great interest in geology and was very helpful to the geological profession. I laid that idea out for him, and he said, "That's fine. I'll publish the series of books." So then I divided the subjects up among the people in the survey, mostly the research people. I said, "I would like to have you agree to write a book on the subject which is your specialty." I think there were about ten of them. I was in the middle, at that time, of writing my book on geomorphology, so that was sort of a prototype of what we were trying to do.

Well, then I took the idea up with the director, and the director said, "That's such a good idea, I want us to do it." I said to the director, "That's not going to work. If it's a government publication, this will not get the kind of publicity we're talking about when we're trying to advertise the Geological Survey. Government publications in general do not receive the kind of publicity and the kind of distribution that a private firm can give it." I said, "Bill Freeman has agreed to publish this." "No," he said, "the idea is too good. We'll do it."

Lage: This is Director Nolan?

Leopold: No, no, this was after Nolan left. Nolan would never have done that. Nolan would have said, "Sure, go ahead." No, this was right after Bill Pecora became the director.

Well, of course the thing fell apart immediately because no one was interested in doing this. They could write a water supply paper or a professional paper any time they wanted to, but the idea of writing a book was something that had never been done before. I was the first one in the Geological Survey who ever published a book while I was in the survey.

But anyhow, the idea fell apart. Now, after twenty-five years, one of the books that was proposed at that time is now in print, or it's coming out. Dr. William Bull of the University of Arizona, who was on our staff at that time, is now publishing a book that was started with that idea. He was asked to write a book on climatic geomorphology, and I understand that his book is

now done and is in press at the present time. So there were lots of ideas that were being kicked around, some of which worked and some of which didn't.

Maintaining Staff Productivity and Initiative

Leopold: The thing that is important about all of this is that somebody who runs an organization of this kind, in my view, will be most successful if he comes at it from a scientific point of view where he's trying to not just sort of flow with the wind but have an independent course: "This is what we're going to do and we're going to stick to this because this is our job." In my view, there's been a tendency for the organization to be essentially directed by the budget, and I think that's too bad.

Now, for example, one of the great troubles that we had, that I think I mentioned before, was that as the staff gets older, they become less productive. I had started the practice that people were going to be moved, they were going to be transferred. Maybe not transferred physically, but at least changed into a new position, and that each year there should be a certain number of younger people hired. Well, until quite recently that was stopped, so the research staff got older and older. No one was moved, no one had their job changed, so you could imagine that the whole research productivity went down the tubes because there weren't enough people with new ideas that were coming up with, "Let's do things in a different manner." I think that's been one of the great changes that--. Let's say that they lost the initiative, they lost the momentum that we had at that time.

Lage: The period when you came was a period of great change and excitement, I can see that.

Leopold: Yes, because we made it so, you see.

Lage: Yes. New ideas, new people.

Leopold: That's right. And new people.

Lage: And how you get that institutionalized and continue the excitement is a challenge.

Leopold: Then it deteriorates. At least that's been the experience.

The Tree Ring Laboratory: Documenting Climatic Change

Lage: There are a few other programs we haven't talked about that I'm hoping you might have something to say about. You mentioned the tree ring lab, the ocean programs, the hydraulics lab, but we haven't talked about what they were, and what happened to them.

Leopold: Something that we had in mind then, something that's been of interest to me for all of my life, is the matter of climatic change, because my concentration on the processes and physical characteristics of river channels and their changes, their changes through time, these changes primarily occur as a result of climatic change. Therefore, the whole matter of measuring climatic parameters is important to geomorphology, not merely to weather bureau people, not merely to meteorology. And of course, at that time this was just before carbon 14 was invented by Dr. Willard Libby. The best procedures available for making estimates of climate of the past were tree rings.

There's a distant cousin of mine by marriage, by the name of Dr. Deric O'Brien, who was an expert archaeologist before the Second World War, worked for a long time in New Mexico and Arizona. He was a Rhodes scholar, took his Ph.D. at Oxford. He came back from the war and went to work basically as a geographer, if you like, an analytical geographer for the CIA. He was concerned with such things as trying to explain to Army people and to CIA people what were the indigenous characteristics of people who lived in some foreign country.

He was moved from Africa to Virginia. I hadn't seen him for many years. I met him and I asked about his work, and he told me about it. I said, "That really is not as interesting as some of the work you used to do before the war. Why don't you join the Geological Survey as a social scientist, and we'll go back to the work that you did many years ago on tree rings, and I'll build you a tree ring laboratory?" Well, he took it up.

This is an interesting story. Deric O'Brien was the son of a woman who was a very strong-minded gal. When he was a young boy, they lived at Mesa Verde, and his stepfather was the director of Mesa Verde National Park. That's where Deric got his first training in archaeology, because he was in on all of the excavation that was going on at that time. So at the age of ten he wrote two books, and they were damn good books. They were not

just children's books; they were very interesting, but they were written by a very young person.

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Leopold: One was called Deric in Mesa Verde, and the other was called Deric with the Indians. Years later, for what reason I don't know, he was always embarrassed any time anything was brought up about the books that he had written when he was a young child. Clearly his mother was very influential in getting those books written. Although she didn't write them, she was very influential. But looking back at it, I have the feeling that Deric was always so, let's say cowed, by his mother, that he never was able to stand up on his two feet and fight for himself. That's my opinion.

Anyhow, I said, "You design a tree ring laboratory of the most advanced sort," and indeed he did. He went all over the United States getting ideas from people who ran tree ring work, particularly the University of Arizona, which is very famous for it. We set up in Washington a tree ring laboratory in which there were microscopes that were set up so that you could look at the tree rings under a microscope and images were thrown up onto a very large screen. You could make accurate measurements, and it was the most advanced in the world.

What Deric and I decided to do was to go to a place in southwestern Colorado and get a series of cores from primarily spruce trees in the climatic region where they're most sensitive to the amount of rainfall. The idea was that we would take the tree ring width for the years of record for which we actually had current records and concurrent crop records from nearby farms. We were going to correlate the width of the tree ring with how many tons of beans or corn or crops that were grown in the nearby area. Then we could go back to tree rings before the present records were available and say, "What could the people have been growing in tons per year per acre, under the conditions of the climate which were indicated by the tree ring?"

So anyhow, the tree rings were collected. They were analyzed in the laboratory, and the manuscript was nearly done. We had at that time also a bunch of high-class mathematicians who were very statistically oriented. Apparently this manuscript, unbeknownst to me, was turned over to the statisticians, and they said, "You ought to make statistical analyses of these data to make sure that they are not random." So there developed a kind of a controversy about whether this work should be published as it was or whether there ought to be additional work done,

particularly in statistics, which is not my field and certainly not Deric's field.

And then furthermore, it was so successful that there were two other botanists on our staff who now wanted to work on the tree ring problems in the eastern United States, and therefore there began to be a competition for the time available of the technician who was actually doing the detailed work in the laboratory.

One day Deric O'Brien walked into my office and he said, "I want to resign from the tree ring laboratory." I said, "What the hell are you talking about?" He said, "There's such a competition, people are imposing on the laboratory and they're pushing me around. I'm tired of it." I said, "Now, that's a bunch of nonsense. The laboratory was made for you. I'm going to kick the guys out." "No," he said, "I'm through. I'm tired of fighting it." I said, "Why the hell didn't you come and talk to me about it a year ago?" "I didn't want to bother you." I said, "Now, look. It's not that serious. I will tell these people that they have low priority, and until you finish the work that you're doing, the laboratory's not available to them." "No," he said, "I'm tired of it."

Well, Deric died about two years ago. I saw him a couple of years before he died, in England, and I said, "Deric, let me finish the manuscript." This manuscript had been held for twenty-five years. I said, "I will pull it out. I will put your name on it, but let me finish it." "No. I will do it," he said. And twenty-five years, and then he died. So anyhow, it was very disappointing. I think that the laboratory has been completely dismantled. Terribly disappointing.

Lage: Was it going to be used for other research as well?

Leopold: It could be used for lots of things, but the thing is that the main purpose for which it was made was dealing with southwestern problems, where tree rings are so critical. But they became overshadowed. An indicator of this is that the work that the other people were doing on eastern problems never received any recognition because they were working on problems that were not very important. So basically, the tree ring laboratory got wasted on problems that were unimportant.

The Hydraulic Laboratories

Leopold: Now, with regard to the hydraulic lab--. When Walter and I divided up the money for research programs, I said, "One of the things that I want to do is to have a hydraulic laboratory in which there would be experiments to determine the roughness--we call it the roughness--in alluvial channels." Now, the roughness in alluvial channels is not determined by the size of the rock but really by the dunes and other bars and things that form in a channel that's made up of fine-grained material--sand, for example.

So two of our men--I think one of them was with the survey at the time, the other I employed anew. I assigned them to Fort Collins, Colorado, where there already was a modest hydraulics laboratory. I said, "You build a flume and work on this problem," which they did. They were very successful and got international recognition for what they'd done. That has had its difficulties too, because these same people later resigned from the Geological Survey and set themselves up as engineering consultants and became very well known, but in many cases their engineering was problematic. Anyhow, we're not going to go into that.

But anyhow, the hydraulics laboratory set up at Fort Collins was quite successful, but it also had some areas that were less than successful. Personality problems, among other things. In addition to that, I decided that I wanted a hydraulics laboratory for my own work, so I finally made an arrangement with the University of Maryland to go to the basement of one of their buildings and build a flume, which I did. It was a very high-class structure. The flume was about three feet wide and about sixty feet long, and it had everything that was needed for good flume work. I had paid to have Brigadier Bagnold come from England to work with us, we devised an experiment, which was a very successful experiment indeed.

I mentioned that I have tried to have one foreign scientist on our staff at all times. Ralph A. Bagnold was a very famous scientist and a very famous officer in the British Army, a brigadier. The brother of Enid Bagnold, the famous playwright. A very distinguished family.

Lage: What was his field?

Leopold: Rivers and sediment transport, but he'd made his reputation in desert studies. This is a long, long story. Last week I went to England for the specific purpose of participating in the memorial

service held at Trafalgar Square in the great cathedral at St. Martin's in the Field, and I spoke at this memorial service for my friend Brigadier Bagnold. I just returned a couple of days ago. That was another one of the great successes we had. We had this great scientist on our staff. I brought him over half a dozen times, at least.

Lage: And you worked out of the hydraulic laboratory?

Leopold: Mostly, we started out by working together in the hydraulics laboratory that I had built for my work. Then we collaborated on many other projects, including the big project in Wyoming at East Fork River. I set up this very fancy bedload trap, which we ran for the period from 1973 to 1980. Bagnold was very important in both helping with the design and also with the analysis of the data.

Lage: Did the hydraulic lab set up an artificial river? How did it work? You mentioned the flume.

Leopold: Yes, it's really an artificial river, if you like. But one of the big problems in hydraulics has always been that the difference between a river and an artificial river in the laboratory is obvious in certain respects and very subtle in other respects. One of the difficulties was that it was not well known at that time what you could and could not do with the artificial river water flowing down a channel if you made it in the laboratory.

Lage: Is that why you moved to this outdoor--the East Fork river--

Leopold: Oh, no. They were two different problems.

Well, then we had only run this laboratory for about a year and a half, I guess, and the University of Maryland for some reason or another said they didn't want us there anymore. So here was this tremendous piece of equipment that had to be moved. So I really had to work hard to try to find some other place in Washington. My administrative assistant finally found me a place in the so-called old gun factory in Washington where they used to make cannons. They had this very, very large, extremely old building that looked like a deserted warehouse. Indeed, though, it had an overhead crane that hadn't been used for anything for a long, long time, and it was an absolutely splendid place. The overhead crane was very helpful, and it was large and had a high ceiling. Bill Emmett and I designed and really built by ourselves a very much better laboratory than we ever had at the University of Maryland.

So I took one of the young scientists that I had hired, and Bagnold and I devised a scheme of what he was to work on in this new laboratory. That was a very successful part. Then when--. Everything was going splendidly. We had not only the big flume there, but there was enough space to make a whole area that could be sprinkled. I wanted to build essentially a homemade watershed so we could have artificial rainfall falling in this large area and collecting it down the mountain. Just as we were getting started on that, Lady Bird Johnson started her famous "We will beautify the countryside." Because of something Lady Bird Johnson said to somebody, they were going to improve this very old building, so they told us to get out.

Lage: What a shame.

Leopold: So having built this marvelous laboratory, it had to be moved to another place, and by the time it was moved, everything had gone to hell and it never was used again.

Lage: Oh, that's too bad. Lady Bird probably never really knew.

Leopold: No, she had no idea. As a matter of fact, nothing ever happened, of course. She didn't beautify it all, and it was actually silly. That's the kind of thing that happens.

The Ocean and Glacial Programs

Lage: How about the ocean program?

Leopold: That's another whole story. There was a gentleman hired by the Geological Survey in the Geologic Division to really put us in the ocean business, because after all, geomorphology of the ocean floor is a very important matter, and oceans are terribly important in hydrology. The one thing that he did during the short time that he worked for the Geologic Division was he said, "There's now just been freed a great oceanographic location at Tiburon in San Francisco Bay. It's free. Take it. You can have it from the Navy." The director said, "No." Because here, in Tiburon, now it's an environmental station, as you know, but at that time it had everything; it had ships, and it had docks, and it had buildings. It was just too big a bite for the director to grab.

There was another aspect of this. I said, "Let's not put all our eggs in that one basket. Let's have a joint program with the Woods Hole oceanographic experiment station." So we sent

several people up to Woods Hole to work there under the jurisdiction of one of the famous oceanographers of the day, whose name is Dr. Emory. So we associated ourselves with one of the best people in the world. That was moderately successful, but compared with what Woods Hole has done before and since, it really was a very small part, so our incursion into the oceanographic thing was too small and too narrow in scope, too narrow in scientific view.

For example, right after that it was discovered by Alan Cox at Stanford, and some associates, the whole business about the Central Atlantic Ridge which at that time had just recently been mapped, the whole question about sea floor spreading, the change in magnetism of the areas parallel to the central mountain range under the Atlantic. We missed out on that. In other words, had we had a larger scope, we might have been in on that, but we weren't.

Lage: You'd need more staff, more money? Is that the idea? Or more inspiration?

Leopold: All those things, I think, yes. It's not always money. It's really a combination of the way people think and their energy and that sort of thing. So again, here was an incursion into a new field, but some of it was extremely successful and some of it was less than successful.

Lage: Did the survey stay with the oceanography program?

Leopold: No. In other words, their collaboration with Woods Hole ended shortly, I think, after I no longer was chief. It was dropped entirely.

Lage: What happened with the glacial program?

Leopold: The glacial program always remained small but very successful. Dr. Mark Meier, whom I hired to run that, turned out to make a big reputation in glaciology. He had only one important assistant, Dr. Post, and I don't know whether they had any technicians or not, but the two of them really did it, and they were very successful at what they did, but it always remained small. I had always hoped that that program could be expanded, but you can't do everything. After I left, the organization didn't choose to try to expand Meier's program.

Influence of Western Irrigators on the Research Program

Lage: You had mentioned, I think, in the video that one of your attempts was to broaden the scope of the research program beyond the interests of western irrigators. I can see that all these programs did that. What did you mean by the interests of western irrigators? Did they exert influence on the survey program somewhat?

Leopold: I see what you mean. A large proportion of the gauging stations were run by the Geological Survey when I came in. They were in the first place in the West, and mostly for purposes of irrigation. Not entirely, but there was quite a concentration of gauging stations in the western United States. Without question, the work on the quality of water was really directed entirely toward irrigation matters because they were interested, basically, in the salts dissolved in water which affect irrigation. In other words, how much calcium carbonate, how much iron and silica and the other things. That's why there was never any consideration of all the other kinds of pollutants, such as heavy metals themselves, things like arsenic, like boron, like fluorine.

And then the whole business of the biological aspect of water pollution. That was not part of the water quality program, so you might say the water quality measurements were indeed directed entirely at the kinds of things that irrigators want to know. But with this expansion of water use everywhere and the pollution problems recurring throughout the United States, I said, "For goodness sakes, let's go beyond that and start biological studies and studies of the trace elements and the other things that are likely to affect water quality."

Lage: Did the irrigators represent an interest group?

Leopold: They represented the people who were putting up the money.

Lage: Through the states?

Leopold: Yes. It was done through the cooperative program.

Lage: So they may have had the influence on the states as to what the state requested?

Leopold: Yes, indeed. Oh, yes. Look at California. I mean, look this week and see what's happening. This morning's paper said that the state is going to pay people two and a half times their normal income for each acre to take it out of cultivation in

order to save water. Irrigated agriculture is a dominating political force in many of the western states. A dominating force.

So as you can see, the problem was to expand the view of what hydrology was supposed to do. In that we were really quite successful, and less successful in some other aspects. We certainly did expand into a lot of things that the Geological Survey didn't do before. Many of these things are being carried on in ways that we couldn't have foreseen. As I said with regard to the biology program, there are something like forty biologists now overseeing the water quality program.

Good things were going on also with regard to instrumentation. I told you that under the influence of Rolland Carter and Joe Wells, who was the branch chief of the Surface Water Branch, we got into new kinds of instruments for recording, so a lot of progress was being made on that. Now, with the influence of computers and radio, it's again changing in a very progressive way.

Lage: Any other programs that we should talk about, or specifics of research and data collection?

Leopold: I'll have to think about that. [tape interruption]

Cooperation with the Geologic and Topographic Divisions

Leopold: One of the things that had been true of the survey in general is that the Geologic Division not only had a different subject matter, but a different way of handling their personnel, as I've told you before. The man might be assigned from geologic work in the field to two or three years being an administrator, and then was put back into the scientific work. That was entirely different than what was done in the Water Resources Division.

In many respects, I tried to emulate that in our procedure. But because I'm a geologist as well as a hydrologist, I have a lot more appreciation for what the Geologic Division is trying to do, so there was a much larger amount of interest and cooperation between our divisions when I became chief than had been the case in the past. The oceanographic program was an example of that. And then I was also trying to move our division much closer to the Topographic Division, which made maps.

One of the things that interested me was the fact that as geomorphologists, we're very much interested in how the river is designated on a topographic map, and the blue lines on a topographic map representing rivers had no real scientific basis. So I went to the chief topographer and said, "I wonder if we could collaborate on the idea that we might try to work out a scheme which would tell you under what circumstances you put a blue line down and some circumstances when you don't, because the blue line ends at some place that's really quite arbitrary."

"Yes," he said, "that would be a very good idea, but you have to keep in mind now that the people who put the blue lines on the map, they're GS-2s, and they have no technical training whatsoever, and therefore, really, the blue line on the map is simply an artistic--."

Lage: That's not very encouraging.

Leopold: I said, "That I would like to change. Let us try to devise a scheme." My scheme was basically this: "I'll try to design a scheme so that the blue line ends at the place where the water is in the stream a certain number of days each year. So it's a very specific, a statistical measure of how often the stream is dry." Well, we did. Walter Langbein and I came up with a tentative plan of how this could be done from our statistical relationships. None of them suited the Topographic Division at all. It was all too complicated because the people that did the actual drawing of the blue line were simply such low-level people that they had a very hard time following detailed instructions.

Lage: Did you have enough data?

Leopold: We were trying to explore it. In other words, yes, we could have done it, all right, but the question was, in what form do you give this to the topographer who's going to draw the blue line? And that's where we never had a chance to develop it to such detail that it became a practical matter. It was completely turned down because it was just too complicated in their view.

Lage: So we can't count on the blue lines when we're looking for water on our hikes.

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Lage: How about the Division of Conservation? Was there any overlap there?

Leopold: No, the Division of Conservation was something we were always very chary about because they did not do any scientific work at

all. The word "conservation" should not have been used at all. They were the people that took in the money in gas royalties for the federal government. That's all they did. In relatively recent years that has been taken out of the Geological Survey and put under the Interior Department itself because it was not the kind of work that the Geological Survey should be doing.

Lage: Did the budget of your division grow in relation to the other divisions during your years? Was there a concerted effort made to increase the size of the Water Resources Division?

Leopold: I simply don't remember, with regard to that particular comparison. We did very well because our cooperative program kept growing, and there was never any question about the Congress being willing to give money to match state money. We never had any trouble with that. The matter of federal funds, I am told just in the last year that the level of federal funds for our research effort is exactly the way it was when I first put it in the budget. It hasn't changed at all.

Lage: It has not changed in all these years?

Leopold: No. I may be not entirely correct on that. That's my recollection of what I was told within the last year. Somebody said to me, "Do you know that the amount of money that we use for federal research is the same as the amount of money you put in?" and I said, "No, I did not."

Relations with Congress: The Senate Select Committee on Water Resources

Lage: What about relationships with Congress? Were you yourself called upon to testify or answer questions informally?

Leopold: Yes. In many respects, yes. Generally, our director, Director Nolan, was so good at dealing with that kind of matter that although we accompanied him to the hill, usually we did not actually speak to the congressional committees ourselves. In some cases we did, but it was not a very important part of our testimony.

On the other hand, when Senator [Robert S.] Kerr started to hold hearings of what they ended up by calling a Senate Select Committee on Water, then I got busy immediately. I was asked to testify before the Kerr people on the Hill. I said to the staff, "All right, now, we're going to have to do this. I'd like to

develop very simple statements of water facts so that Congress can really understand every bit of it." We got a lot of people to work on it in a very short time and came up with a series of slides called the Senate Select Committee slides that were used over and over again, and tried to explain to people the water facts about the United States. It was very successfully done. ["Water Facts and Problems," U.S. Senate Select Committee on Water Resources, 1959.]

But then when the Kerr Committee, after we'd made our testimony, then they went on to do a lot of things that were perhaps less than useful, and ended up by setting up this business of grants for water resources research which were to be administered not by the Geological Survey but by the Interior Department. They set up a special organization in the Interior Department to give this grant money away.

Lage: To give it to universities?

Leopold: Yes. Well, every state was to have, at the agricultural university, a program comparable to the agricultural experiment station, and that was to be called the Water Resources Center. The Water Resources Center in California now is at UC Riverside.

Lage: And the survey wasn't tied into it in any way to coordinate--

Leopold: No, unfortunately they were not. Now they are, because now it has been taken out of the Interior Department specifically and given to the survey, and now the survey has to have an organization to give the money away to the water resources centers. Some of the centers did very good work and some of the centers were less successful. Organizations that had strong interest and scientific work in water such as the University of California, were very successful, and they set up good schemes for determining what research they were going to finance.

Unfortunately, in my opinion--and a lot of people will disagree with this--a large amount of money was set aside and still is set aside to work strictly on desalinization. I would consider desalinization one of the small problems. There are not very many places in the United States where desalinization plants are terribly important. Santa Barbara is apparently coming up with plans to have such a plant. There are at least one or two in Texas. How this program has contributed to the desalinization that goes on the Middle East, for example, in the desert countries, I simply don't know. But I felt that that was not a high-priority matter for our water resources research.

Lage: Do you know what the impetus was for that Senate Select Committee on Water Resources?

Leopold: Yes. Senator Kerr wanted to set up something in Oklahoma that would bring a lot of money into Oklahoma, which he did. He set up a very large research facility in Ada, Oklahoma, which is now run by the EPA. At that time it was being run by the Public Health Service, as I recall. But he was a very powerful man in the Senate, and was responsible basically for the great dredging program of the Missouri River, one of the great boondoggles of all time, it seems to me. There was a time, I know, when the barges of the Corps of Engineers, going up and down the river for the river dredging, constituted practically all the commerce there was in the river, and the big dredging program was supposed to increase the boat commerce in the system.

Lage: Did that happen within your program at all, where Congress would look at your budget closely to see how they could benefit their states? Did they try to get your labs put in certain districts, for instance?

Leopold: No, our operation was just too small to monkey with, really.

Lage: They were going for the Bureau of Reclamation, the Corps of Engineers?

Leopold: They were going for something much bigger.

Lage: So you didn't have this kind of oversight of your budget in great detail from Congress?

Leopold: Congress has become more and more directive, writing into law that this agency will do that, or the National Academy of Sciences will do such and such. It's much more directive than it used to be.

Lage: When you talk about the program being driven by the budget now, what do you mean by that?

Leopold: Remember, now, I'm far enough away from it that what I say is my opinion, but it may not really be the truth and the whole truth. The tendency is presently, in my opinion, to see how the wind is blowing and put in the budget things that they think Congress might want, whether you want it or not. There's this very large program developing, a lot of money behind it, to put in the gauging station water measurement program a large number of parameters, and they're all to be put on computer. It's a very large expansion in the nature of what is to be measured in the gauging station network. I'm simply far enough away from it that

I don't know, but it just seems to me that that is a proposal that was made not because it's scientifically needed but because that's what they think the Congress wants.

Lage: So they don't think in terms of what the science requires.

Leopold: That's my opinion.

Lage: Did you ever have to do what I hear the Forest Service does--a lot of building local support for projects in order to get congressional support? Was there that much politics involved in your division?

Leopold: The main thing was to find out what the states really wanted. I set up an advisory committee consisting of a large number of people from various states that met in our office in Washington to discuss our program with us and see what they thought we should be doing, to make sure we were not simply getting too provincial as far as what we thought the states needed. That was again discontinued after I left. But this was not a matter of coercion, this was a matter of asking them to advise us. Some advisory committees, you see, are useful and some are not.

Lage: Was that one useful?

Leopold: It was useful in a certain way, that the states that were putting up money at least felt that we were trying to be responsive to what they considered to be their needs. So that yes, I think it was modestly useful, yes. I think, however, the director's advisory committee that met in the director's office once a year to advise the director, I thought that was a waste of time.

Lage: What groups were they from?

Leopold: These were prominent people in science and academia, and consulting people. A small group.

Lage: And then would they meet with all the division heads?

Leopold: Yes. Then the division heads told them what they'd do and that sort of thing.

Lage: You didn't get any valuable input there?

Leopold: I never felt that we did. Every year I made a big presentation to the committee as to what we were trying to do, and I never felt that they told me anything that I wanted to know.

Interagency Conflicts over Water Quality

Lage: Can we look now at the question of the degree of cooperation or conflict between the Water Resources Division and other federal agencies?

Leopold: Oh, brother.

Lage: That sounds like a big topic.

Leopold: Yes, that's a big topic. The conflict was, is, and continues to be in the matter of water quality. In those days, the Public Health Service was manned by a group of people that were bound and determined that they were going to take all the water pollution problems under their own wing. The question is, what do you need to measure? Clearly, we were the ones that measured the water, and they felt that they were the ones to measure the quality of the water. But somehow or other, to separate the quality of the water from the water itself seemed rather unreasonable.

So there was continual pressure at the interagency committees and that sort of thing. Really fights, if you like. Disagreements among these agencies as to who should do what. That's where Senator Kerr came in. A lot of these people were very political, and they had very close contacts with people on the Hill, and we did not. One thing Director Nolan was insistent upon, and that is that we had no contact with the Congress at all. Now, good, bad, or indifferent, that was his policy.

There were times when I went to him and said, "Look, we're losing out. Why don't we invite Congressman So-and-so to come and talk to us, and let us show him in our laboratories what we do and why it's important?" "No," he said, "I don't want it done that way. I'm going to make the presentation to Congress, and they're going to do what they're going to do, but I'm not going to appear to cater to Congress."

Well, these other agencies did not feel that way. They went, and they had these congressmen all lined up. The Public Health Service had Senator Kerr, one of the most important people in this area. Therefore, they got a lot of money. They got facilities, they got buildings, they got a lot of things that we didn't get.

Then when the EPA [Environmental Protection Agency] was formed, many of these people who were antagonists of ourselves went over to the EPA.

Lage: From Public Health?

Leopold: Yes. And people that I know tell me that EPA would like very much to take over the Water Resources Division of the Geological Survey. Whether that's true or not, I don't know, but there's that fear. It simply is a continuation of this long fight about what are the important parts of the water field?

I'll give you an example. I got in great trouble with the White House because I was the chairman of a committee, an interagency committee dealing with certain aspects of water.

Lage: This is water quality again?

Leopold: A lot of things about water. Water quality would be involved. We were trying to advise the president's office with regard to certain aspects of what we thought the policy of the government ought to be with regard to certain aspects of water. I've forgotten what these aspects were.

In one of these meetings, I said to the representatives from the commerce department, the Army, the Public Health Service, Agriculture, the other representatives, "There often is a difference between how we, as scientists and as professionals in the water field, think things ought to be done, and how our departments or our organizations think. What I think we ought to do is to advise the White House of the difference between what we think and what our ostensible departments are saying, because in many cases the departments are thinking politically and not scientifically." That was the gist of it. Everybody said, "Yes, that's absolutely right. Let us try to write into this report those things that we ourselves conclude from our own professional experience are the right things to say."

The Public Health Service man said, "I have no intention of doing that. I will give you the line that is dictated by my director, and I will not give you anything else." I said, "Look, that undercuts the whole thing. That's not what we're being asked for. They know what the line is that the bureaus want. They want a scientific opinion." He said, "I won't do it any other way."

Lage: These were scientists from the agencies?

Leopold: They were professionals. They weren't necessarily scientists, but they were the professionals, the top professionals.

So I went to the scientific advisor to the president, Jerome Weisner, there in the executive building. I said, "Dr. Weisner, I am going to have to resign from this committee. The committee refuses to do what I think you are telling us to do, and that is to write a report which represents our best idea, because you don't have to be told what the departments want." Well, he was very angry.

Lage: Angry with you?

Leopold: Yes.

Lage: What was his reasoning?

Leopold: I don't know. I never found out.

And then there was another place I got in trouble right at the same time. The politicians decided that they were going to try to tell the country of Pakistan what they ought to do about the groundwater problem. They asked me to be on this committee to go to Pakistan, and I said, "I know what these people are going to say to you. They're going to talk about drilling more wells. I don't agree with that, and I don't think that you're going to get the thing that's needed. I said, "No, I don't want to be on the committee." The White House was mad at me about that, too. So anyhow, I had my troubles with some of those people up there.

Lage: It sounds like you made a few waves.

Leopold: Oh, yes.

Battling the Bureau of Reclamation over Colorado River Water Quality

Lage: What about the Corps of Engineers? Did the two agencies have, or you yourself have, differences of opinion with the Corps of Engineers?

Leopold: We went up and down. We went definitely up and down, and also with TVA [Tennessee Valley Authority]. We were very influential in some of the joint projects such as the sedimentation survey at Lake Mead in which the Navy, the Commerce Department, and ourselves made this very successful survey. The big problem that we had was basically with the Bureau of Reclamation.

Lage: And what was the core of that?

Leopold: The core of that was that the Bureau of Reclamation did not want the public to be told in any way or form that irrigation makes the water quality deteriorate by the addition of salts, despite the fact that everybody knows about it.

Lage: Why were they--

Leopold: Because, you see, they were building all these big dams, and they weren't about to tell anybody that the irrigation water that they were putting on the land was going to deteriorate the water quality.

I had a contingent of people from California representing a certain aspect of water users, and they came to me and said, "We will back a bill in Congress to direct you to make a study of the water quality of the Colorado River and the effect of irrigation on it, if you do--" what? And I forget what. Or something like, "If we went and got this money for you, would you make such a study?" I said, "You're darn right we'll make such a study. That's exactly what we ought to be doing, controversial as it will be."

So they got the money and we were ordered by Congress to make a study, a complete study of the water quality and the effect of irrigation on water quality of the whole Colorado system from the headwaters down to the Gulf of Mexico. So we put some very competent people on this, and over a period of three years they came up with this thick report. But in order to publish it, the question was, it had to be reviewed. The Bureau of Reclamation stopped it on every count again and again. It took two years.

Lage: It had to be reviewed by the Bureau of Reclamation?

Leopold: It was going to be an Interior Department report, you see, so that the Interior Department went to the secretary and said, "Look, you can't let the survey say these damn things." All we were doing was taking one irrigation project after another and making an estimate on how much water was put on, how much rainfall, how much evaporation, and measured the increase in salt, because we know that the salt increases so much that when you get to Yuma, Arizona, it now has something like 365 parts per million, just on the edge of whether it can be used for irrigation. Gradually, as you go down the river, this salt coming from the irrigation project gets larger and larger. The Bureau of Reclamation didn't want us to say that.

Lage: When would this have been? Do you remember? Was this before the controversy about damming the Grand Canyon?

Leopold: It was still before Glen Canyon was closed. Hoover Dam was in, Glen Canyon was not. I was still chief, so it must have been about 1965. I put one of our most distinguished people on this report review. They went over the thing with this fine-toothed comb to find out every damn place that the Bureau of Reclamation wanted changed. This went on for two years, and finally I went to the director and I said, "Look, director, this is a bunch of stuff. Here's a scientific report that's ready to be published, and we've checked with all this very great amount of effort. Let's publish it." He said, "Okay, go ahead." So anyhow, it was published. A very important report, and should be done again.

Lage: What was it called? Do you remember?

Leopold: When we get back to my office, I'll look it up for you.

Lage: Did it have an impact on congressional discussions of Glen Canyon or the Grand Canyon Dam, do you remember? Or did conservationists pick it up?

Leopold: I don't think it did have much of an impact, unfortunately. The big impact came in another way. The Bureau of Reclamation, in trying to develop an irrigation project, put in a series of wells in a place not far from Yuma, Arizona, called the Welton Mohawk project. They were going to irrigate a large expanse of land down there in the desert near Yuma. I don't remember the details of how this worked, but I recall that they had to pump water out to lower the water table in order to get the drainage system to work. The water table was too high; therefore, by pumping the groundwater table down, then the irrigated water that was put on the land from the Colorado River would have a place to drain out of the drains back into the river. But in order to lower the water table, they had to pump an aquifer that was salty. They started to pump this into the Colorado River right there at Yuma, and of course put Mexico out of business because the water was all salty. Well, that caused a hell of a big stir.

They called a secret meeting, and no one was supposed to know about it, in the Bureau of Reclamation offices in Yuma, I guess it was. I went down there, and there was a big discussion among all of us about the groundwater and the effect of the pumping and that sort of thing. I said to the Bureau of Reclamation people, I said, "Look, haven't you made a statistical analysis of what is the effect of this pumping?" No, they didn't make such an analysis. I said, "Damn it, I'll do it." So I went

out in the hall and I took the doggone data that they had, and I came back in a half an hour and handed this to them, and I said, "Here are the answers to the things that you should have done two years ago." Well, anyhow, it was very bitter.

So immediately after that, President Kennedy flew to Mexico for a big discussion with the president of Mexico about this. The president of Mexico was very, very angry, you see, because here was the salt water coming into Mexico. Kennedy agreed to build a multimillion-dollar bypass to take the salty water past the irrigated fields of Mexico and dump them into the Gulf of California. All caused by one of our government agencies doing something they never should have done in the first place.

Lage: I bet there are a lot of stories like that.

Leopold: Oh, yes, there's plenty of them.

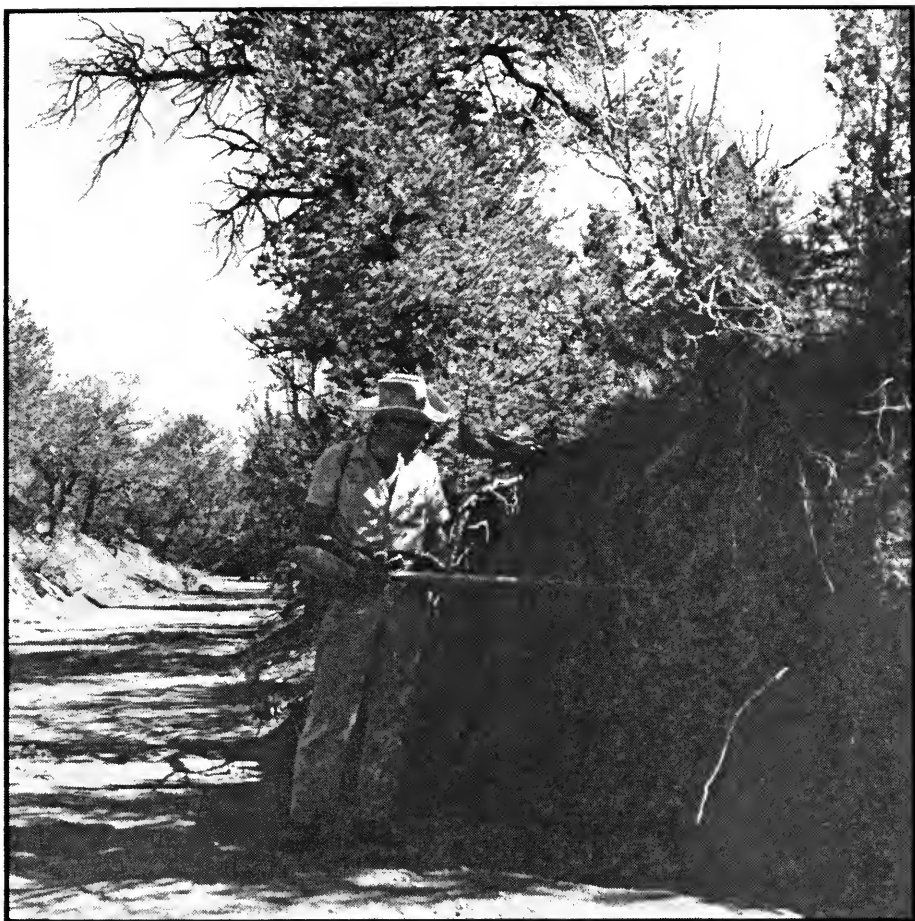


Professor John P. Miller at
Chadron, South Dakota, 1951.



Robert M. Myrick at
Seneca Creek,
Dawsonville,
Maryland, circa 1960.

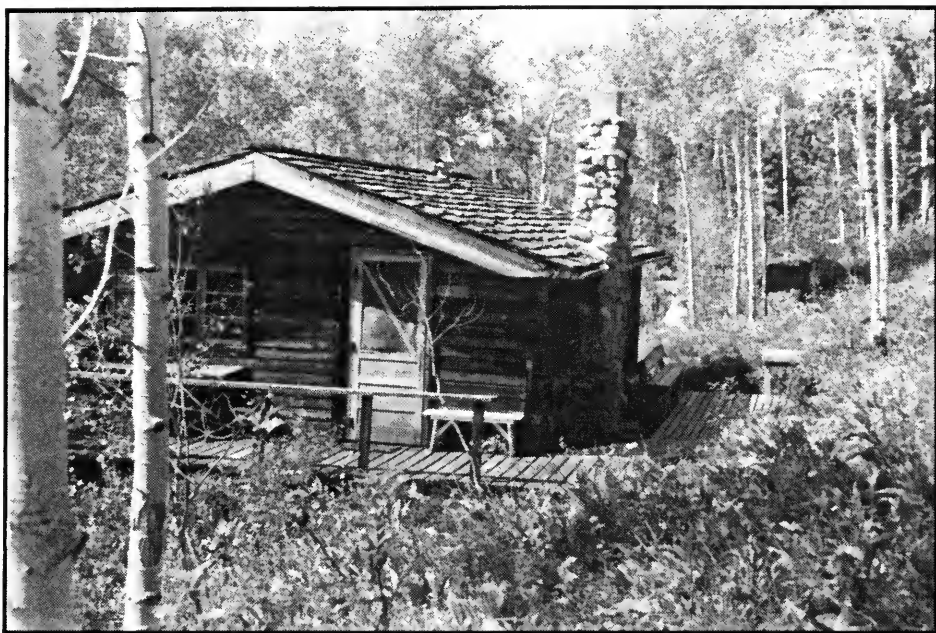
Leopold points to
 prehistoric hearth
 buried in valley
 alluvium, Coyote C.
 Arroyo, New Mexico,
 1966.



Leopold (left) and
 Engineer W. L.
 Heckler, at rock
 group, Arroyo
 Frijoles, near Santa
 Fe, New Mexico, circa
 1966.



Leopold on the East Fork River, Wyoming, circa 1980.



Above: Leopold-Nelson cabin in Wind River Range, Wyoming

Right: After-class party, Teton Science School, Wyoming, 1990.





River trip down the Salmon River, Idaho, 1990.

VIII FUN, GAMES, AND PRODUCTIVE RESEARCH IN THE GEOLOGICAL
SURVEY

[Interview 6: March 5, 1991]##

The Pick and Hammer Show

Lage: We have been talking before we turned on the tape recorder about the fact that our interviews have missed a lot of the fun that comes through so much in your journals that I've been reading.

Leopold: One of the things that we should mention is the Pick and Hammer Club of the Geological Survey, something that was started soon after the survey was organized. It must have been going--I don't know the beginning, but at least at the turn of the century. It's a show, a whole evening's show, written by and acted by members of the survey, and the whole purpose is to throw jokes at each other and to make fun of the people, particularly the people at the top. Some of the shows were sort of written after well-known musical comedies. We had a show of Peter Pan, for example, with all the subjects were changed, and all these people represented the director, and the chief geologist, and so forth. Then, often the club spent a whole show taking off on one person. It was very complimentary in a way--

Lage: Someone in the division?

Leopold: Yes. In other words, it was complimentary in a way, but also very critical in a way. One of the best--. Let's see. I could never forget it. In the first place, you remember that these people are very clever. They have big laboratories and they know how to do things properly. This Pick and Hammer show was about my friend Meyer Rubin. Meyer is a very good-looking man who is a geologist who runs the carbon-14 laboratory, which has all kinds of glassware and beautiful things in the laboratory, you see, and he always wears a white coat.

The second scene goes up, and the stage is perfectly dark. Then all of a sudden you see coming up in a piece of glassware a

yellow liquid which is now starting to go through the glassware. It goes around until finally it circles a woman's body, and then two red lights go on, and then the curtain goes up. Absolutely marvelous.

And then there was a show partly devoted to me. Much of the program was that they were kidding me about my wonderful office. I had my office redone. The show was Camelot, and oh, gee, what they did to some of that. The songs were just marvelous.

Lage: What were they picking on in your office?

Leopold: Well, because I had a very large office and a beautiful new blue rug and new furniture, and everything was really slick, even better than the director, you see. Boy, they could really make something out of that, I could tell you.

Lage: Who were the people who put them on? Was it the same group each time?

Leopold: Everybody. No, all of us.

Lage: Everybody?

Leopold: Oh, yes. Everybody. Everybody wanted to. You could write music. You could play, or you could be in the chorus, or you could do the stage scenery. You could do anything you want, but the point is that everybody pitched in because it was fun.

Lage: And it was once a year?

Leopold: Once a year, yes. In the spring. It was a terrific show.

The Geologic Division pretty much had run the Pick and Hammer Show for a long time, and the man who directed the show for many years was a very talented geologist in the Geologic Division.

Leopold: Then there was always some person in the survey who played the piano to provide the music. We had nothing quite like it in the Water Resources Division. We all contributed to and acted in the Pick and Hammer show, although it was primarily run by the Geologic Division.

Lage: That must have created kind of an esprit de corps in the organization.

Leopold: Oh, yes, you bet. Particularly when they take off the big guys, you see. You felt pretty good when they took you off, because they're paying attention to what you're doing.

Lage: Did they ever border on the unkind?

Leopold: Yes. They often did.

Lage: So sometimes there were people--

Leopold: Some people didn't like it much, but in general, people just laughed like the dickens. Oh, they did one to me that was absolutely wonderful. In the show that was mostly about me, there was an intermission. While they were changing scenery, they put on this movie. The movie was a take-off of an instance that happened in the field.

It was about mid-afternoon, and I had a plane to catch someplace in central Wyoming. I looked at the clock and I said to these guys, all of my young colleagues, I said, "My goodness, there's still two and a half hours to work. Let's go do something." So we all piled in the car and we were traveling across the desert, and we came to a great big wash and got stuck. We were a mile, two miles from the highway, I could tell you. There must have been eight or nine of us, I suppose, and everybody hopped out and tried to get the car out. The car wasn't going to get out, that was quite clear; nor was I going to make my plane. So I said, "What we'll do is I'm going to go to the highway, which was only a couple of miles away, and I'll hitch a ride into town and have a tow car sent out to help you out with the thing. In the meantime, then I'll get on the airplane."

So some of the guys said, "Okay, we'll go with you." So everybody ran for the two miles, and everybody was trotting along. When we got there, here with all these guys, I said, "Hey, I'll never get a ride with all these people standing around. You guys hide in the bushes." I went out there and started to thumb a ride.

Well, you can imagine what they did with that. I'll tell you, when you got through with that, that was the funniest show I've ever seen in my life. Then they had me running for the airplane and all this sort of--. But to see these guys on the stage looking out from these artificial bushes [laughs]--. God, it was funny. It was absolutely a tremendous thing.

Lage: I think you wrote that incident up in your journal.

Leopold: Did I?

Lage: I read that. The jack wasn't adequate in the truck, and then you put on--

Poems. Songs. Literary Allusions

Leopold: What they did in the Pick and Hammer show was absolutely wonderful. Well, then we used to write poetry to each other. Every time that you got an opportunity, you wrote a poem.

Then we had little procedures, or little sayings, most of which I invented, of course. For example, we would start out in some place in camp, and I would say, "All right, everybody." The idea was you took your hat off, you see, and hold it down like this and say, "Hats off to science!" Everybody yells, "Hats off to science!" and off we go. Little things of that kind that were really a lot of fun.

And then, of course, there was a lot of music. I always had a guitar, and we would often play these Pick and Hammer show songs that were very funny.

Lage: So the Pick and Hammer show songs lived on, it sounds like.

Leopold: Oh, yes, you bet they do.

Lage: Did people write them down?

Leopold: They were all published, but many people forgot. Very few people remember back that far to be able to sing them, but they were really wonderful.

I got a letter just yesterday from a man that I had brought over from England to work for the Water Resources Division. I have told you that I always wanted someone from Europe or from another country to be on our staff at all times. I wrote him a few weeks ago--he's now retired--and I said, "Say, what are you doing about literary allusions?" He knows exactly what I mean, because what we used to do is this: I would send him notes about things you picked up in your reading that were little quotes from the literature that you read that had something to do with hydrology. For example, I can remember very well finding one of the best ones I know in Shakespeare. I was writing, you see, about water in channels. Here's one from Gertrude Stein, for

example, that we quote here. This is one of the literary allusions, and here's another one.

Lage: You quote them in one of your journal articles?

Leopold: No, they were really put together for this kind of thing. I was writing a book and I wanted to include them under the chapter titles. Here, for example. Under the title of the chapter is a quotation from Shakespeare. This is about channels, you see. Shakespeare wrote on Venus and Adonis, and you can imagine what he was writing about. "Rain added to a river that is rank, perforce will force it overbank its bank." You see what I mean?

Lage: So you traded those?

Leopold: We traded those, you see. They were referred to in my stuff as "literary allusions." Allusions to something that you're thinking about. Well, George knew exactly what I was talking about. He said, "I'm still collecting literary allusions."

And then there was one poem that I wrote that had to do with a field trip that I took with two geologists and a very famous soils man. Some friends were writing a kind of a little memorial to one of these people that were on the trip. They wrote to me and said, "Have you got anything to add?" I said, "Sure, I'll send you a copy of the poem that I wrote about this trip," again kidding ourselves.

Then there was a Pick and Hammer show about a canoe trip that I took. I took one of my botanist friends. I said, "Okay, we're going to do a canoe trip on the Shenandoah." We drove up to some of the middle parts of the Shenandoah. Four or five days, we floated down the river.

Well, apparently there must have been some things that happened that I didn't remember at all. It had something to do with my drinking, it had something to do with my wanting to sit on a stool or on a log or something. But boy, when they got through with that at the Pick and Hammer show, they really made something out of it.

Lage: And you weren't even sure it ever happened.

Leopold: I didn't remember all these details that this other fellow had recalled, that you could make sound very funny. And they did, too.

Lage: Do you think that still goes on? Is the Pick and Hammer show a continuing tradition?

Leopold: I understand that the Pick and Hammer show these days are sometimes good, but not as uniformly good as they used to be. The Menlo Park office tried to pick it up, the Denver office tried to pick it up, and it just never stuck in those offices.

Lage: You need some talented people, it seems to me, to really put it together.

Leopold: Oh, they were very talented people. My God. Oh, yes. Some of the songs that were written were just absolutely terrific.

Oh, and there were little things that you'd never hear about. For example, I had a practice over twenty years: every time that I published a paper, I brought my secretary a box of candy. Just a little thing, but there were customs that we developed that were very nice.

Field Trips: Canoeing. Surveying. Mapmaking

Lage: Anything in general on your field trips that you'd want to mention? Sort of the fun side of it?

Leopold: When we were in Wyoming for a good long time, after we worked all day we practically always--. When we were working on the eastern side of the Wind River Mountains, and it was always springtime when the water was high, we would run the rivers in the canoe. We nearly lost a couple of guys on one of these trips, but that was the fun. When you got through working about four o'clock in the afternoon, then you'd go about and put the canoe in, and somebody drove around and picked the people up down below.

Lage: You keep referring in your journals to "the river boys."

Leopold: Yes. Because we did a lot of canoeing. Even the bow of the boat had the label "River Boys" on it.

Lage: Were they the same people that went over and over, basically?

Leopold: To some extent. Bob Myrick and Bill Emmett and I were the principal ones. And then there were people that joined us at times. But these were the two people that had been my assistants for a long, long time. Bill Emmett was one of the people I sent to school to get a Ph.D., so he and I had been working together, oh, for thirty years I guess, something like that. Later on, of course, he was no longer my assistant but my equal colleague.

Lage: It sounds as if there was a mix of fun and a lot of observation.

Leopold: Yes. And there was an awful lot to be learned because both of these men, particularly Bill Emmett, were fantastic in surveying, and levelling is a lot of what we did. Surveying. Bill is extremely good on the plane table, but surveying was really what we did. I introduced the idea, for example, of everybody carries the same kind of notebook, and this has spread through all my colleagues and all my students. Everybody carries the same kind of notebook that's done in a certain way.

Lage: How did the journals and the notebook relate?

Leopold: The notebooks are completely technical, and the journals are simply a personal story, if you like.

Lage: But you do have, in the journals, some sketches of how the river is laid out, and a bit about what you're doing.

Leopold: Yes, but the technical data, all of our surveying data, are in the notebooks. But the kinds of things that we did--. Did I ever show you any of the maps?

Lage: No.

Leopold: They're really beautiful things. [takes out maps] Here's one.

Lage: Would you tend to be the mapmaker, or did everybody get in on that?

Leopold: No, I tended to be the mapmaker. The other boys were rodmen for me. But I have always felt that mapping is one of the most important things that we do. So John Miller and I pretty well got this started. This is the kind of stuff that really has to go with the notebooks, because the notes that go along with this are there in the notebooks, you see. This just gives you an idea of the kind of stuff that we did.

Lage: You must have quite a collection. "Forsaken Gully." Were these already named places?

Leopold: Oh, no, many of them I named myself. Oh, no. You ought to see the one in the Czechoslovakian journal. You read this article in Czech; it says, "Dumb Cowboy Wash." [laughter] Another will say, "Meet Mustache Wash." Oh, on and on and on. We made up names for these things. I always told people you shouldn't number things; you should name them. Name them something that reminds you. "Aching Back Wash," for example.

Lage: You'll never forget it.

Leopold: You'll never forget it. But if you had labeled it a number, you'll forget it immediately. But I can tell you about every one of these places where we were, because they've all got names that I know exactly what they meant.

Lage: No wonder you tried to get out in the field frequently.

Leopold: Oh, yes. That was a good time.

First River Raft Trip: Down Lodore Canyon with Herb Skibitzke

Lage: You took a lot of river trips, it sounds like, on rafts.

Leopold: Later on. I told you that Herb Skibitzke was one of my closest friends who taught me to fly. He has been an expert pilot all his life. He flew in World War II for the Navy. When he got through, there just wasn't anything he didn't know about flying, and he also knew a hell of a lot about the Navy.

I decided we were going to take a river trip about 1963 so I picked one of the trips on a commercial expedition. I'd never been on one before, never on a commercial expedition. We'd always just used canoes. But we were going down Lodore Canyon, which is one of the places that John Wesley Powell really lost his shirt. The first part he called Disaster Falls, where he lost a boat relatively early in his trip. And then there followed below that what he named "Hell's Half-Mile." I wanted to see this thing.

So anyhow, we went down there. I had recently sent a team to the Amazon to make the first measurement of the Amazon. They had just gotten back, and they had this big machine that was an echo depth-sounder. I said, "Fine. I'm going to take the echo depth-sounder with me and we're going to measure the depth of this river as we go."

Lage: As you go down Disaster Falls?

Leopold: Yes. So we started out on the trip, and I took this depth-sounder out, and Herb Skibitzke was with me. After that he never wanted to go on a river trip again. We tried to run the machine and it wouldn't run. Well, Herb is an absolutely superb

electronics man, a real expert, so I said to him, "Herb, this thing isn't running. You'd better take it apart and fix it." So at camp that night he took the damn thing apart. It had all this wonderful inside of it that I'd never seen before. He said, "Gee, Luna, without tools I can't fix this thing." He reached in his pocket and he pulled out a little piece of wire about that long, and something that he was holding in his hand like this. He held it to his mouth and he said, "I wish you would send me by parachute" a certain this and that, and then he named the tools that he wanted. He said, "I want them to come into this canyon at 6:15 tonight. He put this thing away.

And, at 6:15, here in this big box canyon, I saw this little airplane come in and I saw the flaps go down, and here it came right over the top of the trees, and out came a parachute with a little box in it. The parachute was made out of a sheet off the motel bedroom. So I walked over there, I picked up the box, brought it back. Herb took the tools out, and he started fooling with this thing. He said, "Luna, it's incredible. This thing will never work. As a matter of fact, I don't think any of this thing is going to work."

The next day, we started down through Hell's Half-Mile. The main boatman, who was supposed to be a real expert, lost the motor, and then he lost an oar, and all of a sudden we were at the mercy of the river. There were two of us at midship. Herb Skibitzke was a great big man, and he and I grabbed the oars, and we started pulling the damn oars. You ought to see the movie of it. My God.

Lage: There's a movie of it?

Leopold: Yes, because at that moment, when we grabbed the oars, the movie camera was still running, and it dropped on the floor of the boat and was still going. You see this thing with the movie camera jumping up and down. It looks up and you see us at the oars. I'll have to give you one of these tapes; it's absolutely terrific.

Well, we got past this terrible rapid, and Herb said to me, "This will never do. Why don't you leave it up to me? I'll get you some boats and we'll run our own."

Lage: He didn't like the way that the outfit ran the boats.

Leopold: No, he didn't like any part of it. But we liked one of the boatmen.

Lage: Was this the Hatch company?

Leopold: It was the Hatch company.

Lage: They did all the early trips, I think.

Leopold: Yes.

At the end of the trip, I went to Smuss Allen, who was our boatman on this trip, and I said, "Smuss, how would you like to run a boat trip for Herb and me? I will try to straighten it out with Hatch." He said, "I've worked for Hatch for a long time, but you straighten it out with him, and if he'll let me go, I will." So I went to Hatch and I said, "I'd like to hire Smuss Allen when you don't use him, and I would like to work out with you your schedule, so that during times when you're not using him at all, then I will hire him."

Well, Hatch wasn't very happy about this, but he said, "Okay." But after the first river trip that we took, then all of a sudden Herb had these boats. Everything was surplus, you see. Everything was surplus. He got everything from the Navy. We had airplanes, we had boats, we had everything, and now we had a boatman. After our first trip, Smuss decided he didn't want to go back to Hatch, so we hired him permanently. So he worked for us.

Lage: You could keep him busy enough?

Leopold: Oh, God, yes, because he had to fix the boats. For example, when we went to Alaska, he hauled the whole thing from Phoenix, Arizona, to the North Slope of Alaska with the boats and everything, for us to take our trip. Herb and I flew up there, you see, but hell, look at how much there was to do. We had to carry all this equipment up there and carry it all back.

Lage: You did a lot of trips in the sixties, it seems from your journal.

Leopold: Oh, yes, you bet. So anyhow, that got us into the boating business. So then we made the thing work, you see. We got a depth sounder that really worked, a nice, simple one. The stuff that we got was just marvelous.

Research on the River Trips

Lage: What kinds of things were you looking at, then, on these Southwest rivers?

Leopold: No one had ever measured the depth of these rapids. No one knew how deep they were. No one had ever really taken the trouble to study them, so that on that big trip down the Grand Canyon of the Colorado (that was my biggest expedition), I think I made six thousand measurements of the depth.

Lage: How did you do that while you were going through the rapids?

Leopold: I'll show you how. Turn that off for a minute.

Lage: Okay. [tape interruption--shows aerial photographs]

Leopold: These are all depths.

Lage: So this is the Green River.

Leopold: This is the Green River. It had never been measured before. What we did was, one person kept track of where we were, and the other person wrote down the numbers that were being read off the machine.

Lage: How did the machine function?

Leopold: What's that?

Lage: How did the machine function? I mean, are you in the boat, and going down quickly?

Leopold: You're going down, and it's sending an echo signal off the bed. A radar type of thing. Then it measures the depth, you see, and it was showing on the dial so that we could read it. I said, "We'll just keep reading it." So they kept reading it about every fifteen seconds or ten seconds. But what we did when I had my air force is Herb went out and photographed these rivers. [shows photos]

Lage: I see.

Leopold: So we'd simply unroll the photographs as we went. We could tell every rock. You could see every bush, so you knew exactly where you were all the time.

Leopold: Then written on here-- . You can't see them yet, but open up a few. This is what I'm giving to the Bancroft. [unrolls photos]

Lage: Wonderful. Would these also be of interest to study how the river changes over time?

Leopold: Yes. Now, here, those photographs were taken in the field. Those are the depths, and you can see how quickly we were reading them. And now I've transferred all of those onto another big map that they've made for me.

There's so much duplication because the airplane has to fly around. It's really quite continuous, as a matter of fact. There. You see you pick it up again on the next photograph.

Lage: Yes, I see.

Leopold: But to have our own airplanes, you see, and then Herb set up a wonderful photographic laboratory in his own office, so these were made in Herb's office.

Lage: And then you got your pilot's license. Did you do a lot of flying?

Leopold: Yes. Yes, we went to Alaska, we went to-- . We went a lot of places.

Lage: Which rivers in Alaska were you--

Leopold: We did a wilderness trip on the John River in the Brooks Range, a very uncomfortable trip, but a wonderful trip. We had a wonderful time.

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Leopold: After every river trip, I wrote a scientific paper about it, a procedure different from that of most field people. For example, this paper is a comparison of two rivers that we ran. One was the John River in Alaska, and one was the Middle Fork of the Salmon in Idaho. This paper, which I called "Observations on Unmeasured Rivers," is to check out to see how much information I could get by simply taking a river trip without any data except what I could observe. I compared that on the Middle Fork of the Salmon, where there's lots and lots of data, but I didn't look at the data until I came back, and I made a comparison between what I could do just going down the river, and what I could do with many, many years of record, and showed that I did very well indeed.

Lage: Which helped you validate--

Leopold: Then you could validate some of the flow characteristics of the Alaska River, where there were no records now, but might be some time in the future.

Lage: You say that you differed from others--

Leopold: Well, because most people go on a field trip and they don't write a paper about it; it's just for fun. When I went on a river trip, I wrote something about it. Here, for example. Yes, here's a comparison of river trip observations against gauging station observations, and you see how close they are. They're really very close indeed.

Lage: Yes, very much so.

Leopold: The purpose here is to show what I could do on a river trip, just going once.

Lage: It sounds like a very valid thing to do when you really have no idea how good your data is.

Leopold: And then our expedition down the Grand Canyon. I wrote this paper about the Grand Canyon.

Lage: "The Rapids and the Pools: Grand Canyon."

Leopold: Here's a picture of one of our boats, you see.

Lage: Yes. "Types of waves and causes of rapids."

Leopold: And then in this paper I discuss the question of what forms the rapids, which I try to describe here. Again, you see, we had an airplane flying over us all the time. Here are the depth measurements in one section of the river, you can see. This diagram shows different kinds of rapids. What forms the rapids under various circumstances.

Lage: How much of the time were you on the side of the river making these observations? The thing that comes to my mind is what a quick observer you are, because I think of the boat just moving down the river and you're picking up all this information. Did you get out and observe?

Leopold: Yes. But for the most part, in Alaska, every time we crossed a tributary we got out and measured it. Now, that took us several hours, but to a great extent there were all kinds of observations that I was making. For example, I was trying to compute the

speed of the water, you see, at different places. There were only a few places in the Grand Canyon where that had been measured. So we learned a lot from our river trips.

John Wesley Powell and the Intrigue of Unanswered Questions

Lage: This is a wonderful book, The Colorado Region and John Wesley Powell. Was this trip made with the idea of contributing to this book?

Leopold: No. As a matter of fact, later on, some years afterwards, Mary Rabbit, who is a geological historian in Washington, had been working for a long time on Major Powell's life history. She and the director, I think, decided that it would be good to publish a book to commemorate the founding of the Geological Survey by John Wesley Powell. So that this book is the commemoration of the foundation of the survey, and it is really a tribute to John Wesley Powell from the survey.

Lage: I notice that you, in your journal, were bringing up some of Powell's observations and trying to prove or add to them. What did you think of his observations after you went down the Colorado River yourself?

Leopold: The paper I'm writing right now goes back to John Wesley Powell and what the geologists have done with his ideas since then. Not much.

Lage: They haven't done much with his--?

Leopold: Well, there are some very important problems in geomorphology that people have skipped over, that were brought up by Powell when he went down the Grand Canyon. The problem of base level, primarily. This is the matter that I sort of followed up, off and on at various times in my life. It's greatly oversimplified in geologic teaching, greatly oversimplified, and nobody-- students are not even told how complicated the matter is because nobody puts it in quantitative terms, which I've tried to do. This is my 150th paper, I guess--the only time I ever wrote a scientific paper in order to say that I don't know how to answer the question.

Lage: That's a good one for your 150th.

Leopold: Yes. I don't know how to answer it.

Lage: But you think it's a question that needs to be examined?

Leopold: Oh, I should say so. A very important question.

Lage: Do you give some guideposts on what directions to take towards an answer?

Leopold: Yes. The problem basically is this. The reason I got into geology is because of this question. When I was about fourteen years old, I was working as a field assistant to a scientist on the Navajo reservation. I was working under an engineer who said, "If we take these gullies that are cutting in this landscape, and we build a check dam, behind the check dam the sediment will accumulate and it will go all the way up the wash until the whole thing is filled up." I said to him, "That's not what you observe. What you observe is that it goes up a little ways and stops." "Well," he said, "you haven't got time. You give it time."

So I put in a check dam myself, and we surveyed it over years.

Lage: Where did you put in your check dam?

Leopold: In New Mexico. John Miller and I put in these check dams, and we surveyed them over a period of years. And then finally I wrote a paper about that several years ago. But we had observed for only fifteen or twenty years, so I went to Israel for the purpose of looking at the dams that were built two thousand years ago by the Nabateans. I showed that they're exactly the same as we saw in New Mexico after five years.

Lage: The same pattern of sedimentation?

Leopold: Exactly the same pattern. That's what I'm writing about now. This is what happens; there was no question about that, and I proved that time is not the problem. The hydraulic problem basically is why the gradient of the deposition is so small. Why is it so small? Fifty percent of the original slope. So that's what I'm writing about, and I'm simply saying, "I don't know." I simply don't know.

So what we did is this: several years ago I asked Bill Emmett to help me. We went to one of my friends in Wyoming and I said, "I'd like to divert one of your irrigation ditches and make a little channel and put in a check dam so I can get some real measurements. Not just what happened; I want to make measurements of velocity and depth and width." He said, "I've already got one in. Why don't you go measure that?" So we went

over there. We now have a set of very carefully done observations on a little check dam, and what I'm writing about now is I'm saying, "Here are the observations, but they don't answer the questions." In other words, I have all the observations I made on this little check dam, but I still can't compute what the slope of the river ought to be.

Lage: So you can't find a pattern that would predict another--

Leopold: No, there's no formula that predicts it. Something is going on that we don't understand. Don't understand at all, and that's what I'm writing about, in the hopes that I can get some young geomorphologist to pick up the problem and say, "I know how to do that." Because I can't do it. I've spent thirty years at it and I still don't know. I've got file after file of studies of this matter, but until we went to this little dam in the field in Wyoming, there weren't any measurements at all. There weren't any measurements of velocity or the depth.

But a friend of mine has a larger dam in southern Colorado, and I'm going to go down there this summer, and we're going to make the same kind of measurements as we did in Wyoming, on a little larger place. It's a very subtle problem, but we simply don't know why it does what it does.

Lage: I suppose that's what makes it all very intriguing. The questions that can't be answered.

Leopold: It's very intriguing, yes.

Choosing the Important Problems in Geomorphology

Lage: Did this kind of study about the sediment have something to do with the dams on the Colorado? Does it relate to the problem that you did get somewhat involved in, or should there be dams on the Colorado?

Leopold: No. That's another problem that's closely related, and still an unsolved problem. Back when I was first with the Bureau of Reclamation, as a matter of fact, there was started this very intensive study of the sediment in Lake Mead after the dam had been built about ten years. When I went down twenty years later, when I went down the Colorado, we came into Lake Mead. I said, "By George, I'm going to see what the delta did," because we studied it years ago. So I made measurements all the way down the river as we went over the delta.

Lage: The delta?

Leopold: The delta is the underwater deposition, which also turns out to have a gradient about 50 percent of the original slope of the river. The tope of the delta is underwater. So really, nobody's measured that either. Nobody's even taken the trouble to make these measurements.

Lage: And they seem so crucial.

Leopold: They just don't seem to be able to pick important problems, in my opinion. That's in geomorphology. Though I found that the sediment had indeed, the front had moved down the Colorado River about twenty miles, I guess, something like that. But I mapped it all the way to the end. And no one's ever taken velocity measurements to see what happens. And again, no one's asked the question, why should the deposition be at that gradient? Nobody knows.

Anyhow, there are some very important problems in geomorphology. For example, I have said to all my students--I don't think anyone's ever done it--I said, "I would like to suggest that you do the same thing I do and have a private file in your office that's labeled "Idea File," in which you keep a record of what you currently think are the most important problems of your science. Every once in a while take that file out and ask yourself this question. Keep in mind what I'm telling you, that you can waste your life on three small problems. Every once in a while you ought to go back and say, "What do I think is really important to work on?" Here are two problems that I have in my file, and have had for thirty years. No one's ever worked on them. So it's peculiar.

IX INVOLVEMENT IN ENVIRONMENTAL ISSUES AND ORGANIZATIONS

Basic Hydrological Research and Environmental Problems

Lage: Do you pick problems at all because they relate to, say, environmental issues, or is that not the reason? I mean, is the reason not that practical?

Leopold: No. Some of the things that I've done for the environment are simply outgrowths of something I've done elsewhere. It really works the other way. You can't solve environmental problems without knowing something about basic process. My job has always been basic process, and I can then apply our findings to things of an environmental manner.

Lage: But the basic process is the main--

Leopold: That's the difficulty. You take the problem of global warming, you take the problem of the ozone layer, weather forecasting, you name it. We were wonderful at now being able to make observations, but we may not be keeping up with our theoretical knowledge of why this phenomenon is as we see it. I just mentioned in my field two of the phenomena which are simply not known.

We still argue about global warming. We've had beautiful observations on the increase of carbon dioxide, but we are not really quite sure now how our mathematical models of the climate take that piece of information and turn it into a result that you can operate on. As you know, there's a lot of discussion, and the present administration doesn't want to pay any attention to it. They say it's like acid rain; we need more research instead of going and doing something about it.

Lage: You mentioned the global warming in one of these Sierra Club papers I looked at, in one of the Wilderness Conference books. I hadn't realized that people had been talking about it that far back. You were asked a question about the effect of man on

climate, and mentioned the increased carbon dioxide back in, oh, it must have been '59, I think.

Leopold: You see, practically my whole life in geomorphology has concentrated on the effect of climatic change, what's happened to rivers as the climate changed. That has been a very large influence on my scientific work. So as far as I know, and I don't mean to claim credit for it, as far as I know, I was the first one who ever brought it up.

Lage: Brought up the global warming?

Leopold: No, the question of what would you do with the water supply problem if you had a change of climate? I said this in a conference at La Jolla, back in about 1956. Now lots of people have gotten on top of that, but as far as I know, I was the first one who asked that question.

Lage: The question that we're dealing with in California now?

Leopold: Yes, exactly. One short piece that you might be interested in reading is the one that I called "A Reverence for Rivers." Did you ever see that?

Lage: No.

Leopold: It's a little, short paper. I was asked by Governor Brown to give the keynote speech at the governor's drought conference in 1977. He sent Stewart Brandt to talk to me, and I asked Stewart, "Why does he want me to do it?" "Because he knows that you'll say something that he would like you to say but he won't say it." So I said, "Don't build any more dams. Start conserving your groundwater," and a few things like that. Of course, nobody paid any attention to it. It's only a one-page paper. It might be of interest to you.

Agency Politics and Dams on the Colorado River

Lage: Let's talk a little more about your relationship with the environmental movement. I came across something in the Sierra Club Bulletin (1967) where David Brower, in an article or maybe it's a speech about the Colorado River, says that he was told that the USGS couldn't make sedimentary projections on the Colorado. They were forbidden to make them. Is that anything that rings a bell?

Leopold: Yes. You're quoting David Brower?

Lage: Yes. Who was quoting Hugh Nash.

Leopold: Who was quoting me.

Lage: Okay. What was that all about?

Leopold: The problem was, you see, that again and again, the Geological Survey is pointing out scientific facts that the Bureau of Reclamation doesn't like because it was going to interrupt their development plan. You know the story of the Teton Dam. A year before the Teton Dam failed, where they had \$3 billion dollars in loss, there was a memorandum written by more than one geologist which was aimed at the Bureau of Reclamation saying, "Look, this is wrong. You're putting that dam in a very unsafe place."

This always happens in a department such as Department of Interior. There's always a tendency to, at the top level, not to allow the bureaus seem to compete with each other, nor to be critical of each other.

The problem came up in this way. Dave Brower finally agreed, and he's been very unhappy with it ever since, that if they would not build the dam at Echo Park, in Dinosaur National Monument, that he would not have the Sierra Club object to building one at Glen Canyon. And of course, we've been sorry about that ever since, but anyhow, that's what we knew at the time.

Lage: Do you know anything about why he made that agreement? Were you involved in that at all?

Leopold: Yes, because we were all very much interested in saving the national monument, but no one had ever thought what would happen if you put another dam in the Grand Canyon. Then later on, when they saw what was going to get flooded, then everybody was sorry. But there were a lot of other reasons--legal reasons and administrative reasons--why the dam was probably going to be built no matter what any of us said.

Lage: Did you advise Dave Brower in the matter of Glen Canyon Dam?

Leopold: No. Anyhow, as the water rose in Lake Powell behind Glen Canyon Dam, the water was going to come up practically to the base of Rainbow Bridge, the great sandstone arch, the most famous in the world. Dave Brower was looking for ways to--and Rainbow Bridge was a national monument again--for ways to protect it. He came

to me, or wrote to me, or talked to me, and said, "What do you think?"

In the first place, in some manner or another, a geologist was sent by the Geological Survey to look at it, and he wrote a report, again which the Bureau of Reclamation didn't like. That was kind of squashed. There was some talk about whether they ought to put a dam in the canyon downstream from the arch to keep the water from going up to the base of the arch. The question Brower brought to me was, what would happen if we put a dam there? I replied to him that I couldn't do this officially. So this was all done sub rosa.

Lage: You couldn't comment officially?

Leopold: No. I couldn't make an official statement because the Bureau of Reclamation would raise hell with the director and all that sort of thing. By private communication, I told Brower what my ideas were so that he was getting a certain amount of information from the survey. The directors of the survey have always been very squeamish about facing off other bureaus in the Department of the Interior even for good scientific reason. Unfortunately, you see that there's an awful lot of things that go on in government where even agencies in the same department are really doing opposite things and they're doing things that are clearly antithetical.

So I made an estimate for the Sierra Club in an unofficial way on the Rainbow Bridge matter, and it didn't come out in the way that all this trouble has come out recently. The Geological Survey got themselves in a hell of a spot, you know, a year ago.

Lage: Regarding Rainbow Bridge?

Leopold: No, no. Regarding advising organizations on scientific matters. There's a member of the Geologic Division whose name is Howard Wilshire, in Menlo Park, and as I understand the story, he had been working, as many people had, on the question of off-road vehicles and their effect on the desert. Someone in one of the conservation groups asked Wilshire to go on a field trip for one day, which he did on a weekend, on his own time, to talk about the problem of what his research had shown.

I think it was the Forest Service that brought the matter up to the director of the Geological Survey. The director ordered Dr. Wilshire to not only cease and desist, but he was going to take him off the payroll for a month and give him an official letter of reprimand, so this has become a great issue now as to what is freedom of speech. So now within the last couple of

months there now is an underground letter formed in the Geological Survey to report on this whole matter of freedom of speech and what you can and can't do. Boy, the scientific community went up in smoke about it. They said, "Look, the man's giving his discussion on his own time on things that are not affecting the Geological Survey." The director never did back down, but he got in a hell of a lot of trouble.

Testifying in Arizona vs. California

Lage: Did things like that come up with you also?

Leopold: It did in one respect, but that was a little bit different. About 1958 I was asked to testify before the United States Supreme Court in the case of Arizona versus California, the most famous law case ever tried in the field of water.

Lage: What was that about?

Leopold: The case concerned what water in the Colorado system was to be included under the 1922 compact, what water rights do the Indians have, what water in ephemeral streams is included, and other issues.

I was asked by California to appear, and I said, "I will appear but it may not help you at all. I will talk about the facts as I know them, but whether it hurts California or helps California, or hurts Arizona or helps Arizona, I'm not able to say." But I said, "Further, everything that I say I'm going to publish in the scientific literature, and only on that condition will I appear." So I did, and the paper which I wrote in that testimony is called "Statistical Methods applied to a Water Supply Problem."

This raised a hell of a big to-do because the Salt Lake City paper had a banner headline saying, "Leopold Takes Two Million Acre-Feet Out of the Colorado River." The governors of several states sent delegations of their state engineers to Washington to talk to the secretary of the Interior about discharging me.

Lage: What was the gist of your testimony that was so controversial?

Leopold: That the Bureau of Reclamation should have known that their estimate of the amount of water in the Colorado might have been wrong, as it turned out to be wrong. I said there was a reasonable chance that the figure was 13 million acre-feet or 17

million, rather than the 15 million acre-feet the Bureau was estimating. So the Salt Lake City newspaper said I took 2 million acre-feet out.

I was telling the Supreme Court that the numbers could be wrong, you see. Everything that the Bureau of Reclamation had argued about was dependent upon their estimate of how much water there was. But the records have shown gradually that they were wrong, that they were overestimating. So that when I said to the Supreme Court, "Look, the number is probably not right and that can make a lot of difference," well, they had this conference with the secretary of the Interior.

Lage: It was Fred Seaton at the time, as I recall.

Leopold: Seaton, yes. In the next couple of days, Seaton came out with a press conference in which he said--and no one's ever said it before or since--"The Geological Survey is a scientific agency in which they're supposed to give their best opinion. That's what Leopold did, and I'm backing him. You're not going to fire him." A very clear statement about what that secretary felt the survey ought to do.

Lage: Was there support to get him to make that statement? Did you yourself have to appeal to him, or your director?

Leopold: I don't know.

Lage: You yourself didn't?

Leopold: I did not, no.

Lage: But there may have been someone behind the scenes.

Leopold: I don't know the details of how that came about.

Lage: I would think that kind of pressure would create an atmosphere in the agency that would really stifle intellectual freedom.

Leopold: That, of course, was one of the things that I was fighting for all this time, to see to it that people were given a chance to write what they thought, regardless of what the department said, and for the most part, it was very successful.

Pressures for River Development vs. Scientific Fact and Public Interest

Lage: Did you get any more involved in advising the Sierra Club on the Colorado River issue?

Leopold: Yes, indeed. We weren't advising the Bureau of Reclamation, I can tell you. We were writing scientific papers. Yes, indeed.

As a result of my testimony, my friend Walter Langbein continued to work on that same problem, and he wrote a paper that was a real eye-opener to everybody. He showed, from scientific analysis, that by continuing to build more storage dams you don't increase the amount of water at all, but you decrease the amount of water, which was the opposite of what everybody thought. The reason is that in the problem of providing storage, the purpose of storage, as you imagine, is to smooth out the record so that in dry years you can take some of the stored water and increase your supply. You store it in wet years, water that you don't really need now. Now, as you make more storage, then you are basically increasing your supply by storing water so you can use it in a later time, but the more storage you build, the less efficient it is.

Lage: The more evaporation?

Leopold: And you get to the point where the slight increase in the amount of water is balanced by the evaporation, so that you start losing instead of increasing. No one ever said that before. Here was something that really flew in the face of the Bureau of Reclamation, you see.

Lage: Isn't that point something that Brower used rather extensively in his arguments against Grand Canyon dams?

Leopold: Yes, that certainly came up.

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Lage: Did you have any dealings with Floyd Dominy [Commissioner of Reclamation]?

Leopold: Oh, yes. Not personal dealings. I can remember one time I was on the airplane, on a commercial airplane, and close by me was sitting the famous Congressman Aspinall from Colorado. He was traveling with Dominy. I heard them say, "Oh, yes. There's that son of a bitch Leopold." They knew that what we were doing at

the survey was not to their liking at all. Personally, I had practically nothing to do with him.

Lage: You didn't have a professional contact?

Leopold: No. I met him in meetings and that sort of thing, but no, it was not that.

Lage: There was no love lost, it sounds like.

Leopold: Well, because people like that, you see, don't want scientific facts, because it gets in their way. You can see this all the time.

Lage: Dominy seemed to have kind of a crusading spirit about all of this.

Leopold: Indeed, yes. A hell of a lot of those reclamation people did.

Lage: What was their--

Leopold: Well, because there was a group of western congressmen who really wanted money spent in their state, and the one way you could get money, of course, was to build dams, and you'd get a lot of money. So that a tremendous amount of federal money was put into states in the water program that was running between about 1960 and 1975 or 1980, and as it gradually became clearer that there were a lot of troubles with doing this, then the Congress began to back away from this. They thought, "This is not really as good as we thought it was going to be." With the environmental movement, even the Corps of Engineers got backed up to the wall.

But when you had people like Senator Kerr and Congressman Aspinall, people like that who are really getting a lot of mileage out of federal money spent on water, the Corps of Engineers was going wild and so was the Soil Conservation Service, so was the Bureau of Reclamation. It took a while to find out that this had many disadvantages.

Then as the environmental movement got under way, and this came more to the public's attention, the public began to perceive that this is not the way they really wanted to spend their money, just as now in the question of timber harvesting the public is beginning to see that if you chop down all of the old-growth forest in the Northwest, that there's going to be a tremendous loss to the public in some manner or another, even if people can't quite see exactly what that loss is going to be. That's a big shift, you see, in the public attitude.

Advice to Secretary of Interior Udall

Lage: You had some discussion with Secretary of Interior Udall, you had mentioned, regarding the Grand Canyon.

Leopold: Yes.

Lage: Tell me a little bit about how you came to know him and what--

Leopold: In the first place, you understand that my father's writings were very famous, he was a famous man, and Stewart Udall felt that he was going to be the conservation secretary of the Interior, and indeed he was. He did some wonderful things. Later on Mo [Morris] Udall more or less took it over, and he became the conservation spokesman in the Congress. But because they're both from Arizona, they both, as well as Senator Goldwater, were backing a dam at Marble Canyon in the Grand Canyon, and they were going to push it through. It was about 1966, it must have been.

Lage: Were you still chief?

Leopold: Yes, I was still chief. It was rather embarrassing. I think I told you that Udall knew me, but he didn't know my director. I would often get a call from his office saying, "Come on over for lunch," or something, but then I had to rush up to the director and say, "I don't know what he's going to talk about but here's what I think, and I'll be back immediately and tell you what happened."

Lage: So what kinds of things would he talk to you about?

Leopold: Well, this was one. I came into his office and he had his feet up on the desk and he was chewing an apple. He said, "Luna, you've been down the Grand Canyon. Is it worth saving?" I looked at him and said, "Well, Mr. Secretary, if you want to save it for a bunch of damn tourists who are going to mess the place up," I said, "no, it's not worth it. But if you are going to make a real name for yourself as a conservationist, which I know you'd like to do, the one thing that will make you famous for the rest of time is if you come out against the dam in the Grand Canyon." Then we talked a little about it, and sure enough, two weeks later he came out in public and said he was against the dam. That killed it. Without the secretary of the Interior in favor of it, it was not going to go.

I talked to him about quite a--. That was certainly the most dramatic, and I'm not sure how much say I had. Maybe he was leaning to it, but there was no hint up to that moment that there was anything except complete support for the dam. I think that I may have just pushed him over the edge, that he saw that that was not going to be to his credit.

Lage: Can you remember other things that he'd call you in about?

Leopold: [laughs] I can remember more than once I got a call that he wanted to have lunch with me. He had a lunch room that was a very, very large room with a very, very long table, and very often we would be served alone in this very large room. The next day I got a bill: \$2.20 or something like that, for lunch.

Lage: You're kidding.

Leopold: I couldn't believe it. I don't know. For some reason. The things that happen in the government are just--

Lage: [laughing] That's wonderful.

Did you get so that you felt friendly with him, or was it always kind of a formal--

Leopold: No, he certainly felt very friendly to me, called me by my first name. I never felt like I could talk to him by his first name. I talked to Mr. Secretary. But I had lots of contact, of course, with people on his staff at lower levels. Immediately below him, and his personal staff, and the undersecretary, and the deputy undersecretary.

Lage: Are there particular ones that stand out as being either especially good or especially bad?

Leopold: Yes, sure. Nathaniel Reed from Florida, of course. I'll think of some other names. One of my own staff, Frank Clarke, a very close friend of mine, was asked to be the deputy undersecretary, I think it was under Nat Reed, as a matter of fact.

Lage: Which actually was later; Reed was there under the Nixon administration.

Leopold: Yes, of course, and that's when I got into the Everglades jetport.

Lage: Yes, which we're going to talk about, but let's finish up on the Grand Canyon. On kind of a personal note, I notice that your

daughter had given congressional testimony about the Grand Canyon?

Leopold: Yes. She made a big impression. She had been down the Grand Canyon. Not on a trip with me; on another trip. She was about thirteen or twelve or something like that, and she took it on herself. She got admission to this hearing, got a place on the program.

Lage: On her own?

Leopold: All by herself. I remember a couple of days after that--I didn't hear her talk, I read her talk--I was flying with Herb. We landed at Edmonton, Canada--we were on our way to Alaska--and I heard my name called on the paging system. I went to the telephone, and a person said, "Senator So-and-so wants to talk to you." I said, "Very well, put him on." So he said, "Are you the father of that girl that I heard a couple of days ago testify for my committee?" I said, "Madelyn? Yes." "Well," he said, "that was very impressive. She must come from somebody who knows something. I wonder if you'd give me some advice," and on and on and on.

Lage: He probably didn't realize she'd done this all on her own.

Leopold: I don't know, but I made it pretty clear that I had nothing to do with it.

The First Environmental Impact Review: Everglades Jetport

Lage: Shall we talk about the Everglades jetport? That sounds like a very interesting tale involving a couple of different government agencies. That was in 1969.

Leopold: Yes, that must have been about '69. I knew very little about the Everglades problem, very little indeed. I was in Pinedale [Wyoming] and I got a call from the undersecretary saying, "I want you to do a job for me. I want you to go to Florida and look into this whole business that we're very concerned about. The Department of Transportation wants to build a big jetport there which is going to be larger than John F. Kennedy jetport in New York. We don't know what our position ought to be. We'd like to have some advice." I said, "I'm busy, but if you'll wait a week or two, I'll get over there."

Lage: Was this Russell Train, by chance?

Leopold: Yes. So I was told then by the deputy undersecretary that this was going to be a joint report between the Department of Transportation, which was really behind the jetport, and the Department of Interior. Interior was involved because it was so close to their land.

Lage: To the [Everglades] national park.

Leopold: Yes. Well, a couple of weeks later I arrived there in Miami. They took me to the park and flew me around in an airplane. They put me in a jet boat, and we went all through the Everglades. Then I sat down and talked with all these people that knew different things: the Fish and Wildlife people who knew about birds, the Fish and Wildlife people who knew about big game, some of them who had experience with fire, and then there were people who had experience with the whole business of water. I wanted to know about alligators, I wanted to know about the special species that occur only there, like the Florida kite and things like that.

So I said, "Very well, I see now what we should do. I have to go back to Wyoming, and since all of you are really the experts, I'd like to have you do this. I'm going to assign portions of this to you, and when I come back in about three weeks I want you to have written something on the order of three pages on these things on which you are specialists." And I assigned all these things.

Lage: Did you get to choose these people, or were they--

Leopold: They were all the people that were there. The people from the Interior Department, people from the Park Service, people from the Fish and Wildlife Service, people in transportation. I wanted to know, for example, about the chances of killing birds with an airplane. Not only killing birds but killing people, if a big bird got in the engine. So I assigned these things. I said, "Here's what I'd like to have you do." All these people were experts, you see, so there should be no problem.

I came back in three weeks. I got them all together and I said, "All right, now if you'll just hand me everything that you've written, then I can start discussing with you how we're going to edit it." I looked around, and nobody had written anything. I said, "Wait a minute. This is August, and do you know that the report is due on October 15 or something like that? We only have six weeks to go to write a major report." I said, "You haven't done anything?" No, they hadn't done anything.

Lage: Did they have a reason? Was there something behind it?

Leopold: No. Simply they just didn't get to it. So I turned to the chief man and I said, "All right. I want three secretaries full time. I want a dictating machine. I want typewriters. I want an artist. I'll write the report." So I sat down with a dictating machine and I wrote a report.

Lage: Based on the verbal--

Leopold: --most everything they told me. And, I'd seen a lot of stuff. I remember I was in the middle of writing this, and everybody else was looking at the television in this motel room. I walked up to the room where they were watching, and I saw Neil Armstrong taking the first step on the moon. I said, "I haven't got time to look at that." I went back in the room and continued my work, and in a day and a half I had a report written.

The wife of one of the men was a graphic artist, so I said, "Okay, I'm going to make some sketches. Here is what I want to illustrate. I want to illustrate fire and its relationship to alligators, relationship to deer." I made out these sketches, and I said, "I want you to put these in final form. Redo them but in a nice way." She was very good; she did a good job. Within three days, I had a report written, so I got back to Washington.

Now, I had written a report but I had nothing about transportation. I had lots about airplanes and lots about pollution and lots about water and lots about birds and wildlife, but nothing about how many airplanes, how much transportation, so I called a representative of the Department of Transportation and said, "Well, you know, now, we're only two weeks away. I've got to have your input so I can work it into this edited draft."

Nothing happened, and nothing happened, until I made several telephone calls. Finally the day arrived when the report was due. The day before, I called this guy. I called him into my office and I said, "Look, you have on my desk, tomorrow morning at eight o'clock so I can do it tomorrow morning, whatever you're going to submit. If you're not going to give me anything, I'm going to take your name off the report. It's not going to be your report at all. It's going to be my report." Eight o'clock came and nothing happened so I took his name off the report and had the top cover page retyped and sent it in to the secretary of Interior. Well, this was the first environmental impact statement, and it made quite a hit.

Here's where Nat Reed came in. Nat Reed at that time was the scientific advisor to the governor of Florida [Claude Kirk]. I think I told you that without the backing of the governor of Florida, we would have gotten nowhere. Nat Reed persuaded the governor that he was going to be smart to go along with this report and say, "No, we're not going to have this jetport here," and he did. So there Nat Reed was very important in utilizing our report to persuade the governor, "Don't fight it. Go with it."

Lage: Was there any such thing as an environmental impact statement, then? That was just about the time that NEPA had been passed.

Leopold: No, that was it. That was the original.

Lage: So that was found to be useful as a model, then.

Leopold: And was useful as a model. As a matter of fact, that's one of the problems, is that they copied my report. The Council on Environmental Quality required thereafter that everybody use the same format that I used.

Lage: Russell Train then became head of the Council of Environmental Quality.

Leopold: I'd forgotten that. Previously, he was head of the Conservation Foundation.

Lage: Yes. Did you use the method of looking at the relative impact of different alternatives courses of action?

Leopold: Yes. Definitely. Yes. Exactly. [tape interruption]

I must have bound it in here. Yes.

Lage: "Environmental Impact of Big Cypress Swamp Jetport" [U.S. Department of Interior, September 1969]

Leopold: And that was where the name came from.

Lage: Even the name "Environmental Impact."

Leopold: Yes.

Lage: That's fascinating. Do you know the path by which this became the model?

Leopold: No, I don't. No, except I know that the CEQ had a lot to do with it.

Lage: They were probably bustling around trying to think of how to deal with NEPA.

Leopold: Probably.

Lage: Did you have a model as you went through this, sitting down and dictating the report? What was your conception of what it should be, or how did you--

Leopold: I was simply trying to write a report on what I thought the jetport was going to do, what were the advantages, what were the disadvantages, and what was going to be the final outcome. The main thing is that the final conclusion was picked up by the newspaper, you see, and spread all over the map in Florida. Those were the drawings that I made that I asked this lady to prepare in a little better form for me, but I made the original drawings, showed her exactly what I wanted, and I had her just do them.

Lage: [Looking at report] From what I've seen of your drawings, I think they're the equal of these. Graphs. It sounds like the jetport would have been a real disaster.

Leopold: Yes. Oh, it would have, no question about it, yes.

Lage: And you hadn't been that aware of it before you were called down there.

Leopold: No, no, I learned everything that had to be learned when I was there, in just a short time.

Preventing an Ill-Conceived Trans-Alaska Pipeline

Lage: Just before this, you got involved in the Alaska pipeline. How did that happen, and what did it involve?

Leopold: That was even worse. Alaska was required by law to prepare a plan of the whole route of the pipeline, which was to go up the John River, which was the river that I had taken this expedition down.

Lage: Had you taken the expedition on the John River for this reason?

Leopold: No, not at all.

So they had taken all these survey maps, and they showed the route of the pipeline, and all these maps had to be folded. When they got done, there was a stack about this high [indicates]. There was an assistant to the director of the Geological Survey that I didn't know very well, who was kind of a liaison for the director with the Department of Interior across the street. He had been following this matter, and apparently he brought it to the attention of the secretary that the Geological Survey better review this report before the permit was given for the building of the line.

All right, this is what happened. He phoned me from the director's office and said, "They've got this report and I'd like to have you read it for us and tell the director's office what you think. Should we make any objection to the issue of a permit?" I said, "Send it over." Well, I didn't know what the report was going to look like, but they brought on a cart like this and stood it in my office. It stood about this high [indicates height].

Lage: Three feet.

Leopold: Oh, at least. More than that. Four and a half feet. So I said to the man, "How much time am I given to review this report?" He said, "Fifteen minutes." I said, "All right, sit down." So I took the top volume off and opened it up, and the only thing I wanted to see was a cross section as to what they were going to do with that pipe. The cross section in it was about this large [2 inches] and it showed a circle for the pipe, ground surface, and some dots showing gravel, and that was it. No dimensions, nothing.

So I said, "Thank you. I've reviewed the report." I put it back, and I said, "You can take it back to the secretary." So I went up to the director's office, and I said, "This is one real disaster. They have never heard about permafrost. They have not even thought about how they're going to cross the river." I said, "I've been on this river. I know what this river looks like. It's going to be a mess."

Lage: You could tell just from this quick look that it wasn't--

Leopold: Well I mean, if that's all they had, they didn't know what they were doing.

The director then said to me, "Very well. You'd better go take a look at this if it's that serious." I said, "All right, I'll take a look at it, but I'm going to choose the people to write the report with me, and we're going to do it on our own. I

want Herb Skibitzke on our own airplane, and I want Bob Curry." Bob Curry was a friend of mine who had a degree from this department [UC Berkeley Department of Geology], who was teaching at that time at Santa Barbara. Bob had written his thesis on the High Sierra, so he knew a lot about ice and he knew a lot about frost action and he knew a lot about permafrost and that sort of thing.

So anyhow, Herb and Bob and I got in our own airplane and we flew up there. Well, you ought to see the pictures. We were flying over the places where the bulldozer was making the track there that ran up the river.

Lage: They were already started?

Leopold: Oh, they were started, all right. You could see the bulldozer come up to the river. It wouldn't know what to do, so you'd see a bulldozer knocking down trees over here and knocking down trees over here until he found a place to cross. He'd cross the river and then go on. Here was this bulldozer track going on and on and on up this river.

Lage: And you were taking photos from the air?

Leopold: Oh, you bet. We had started out from a place called Crevice Creek where we had met a man who was from the eastern United States who had gotten tired of civilization. He moved up there in the middle of the wilderness in the Brooks Range near this river that I was talking about. He married an Eskimo girl, had two little children that they were teaching themselves, and he was making his money by taking people in his little light plane to go hunting up there.

So at this little airstrip, just a little gravel strip in his front yard, we landed our airplane there and we went to see Bill. We said, "The damn bulldozers have gone through your place a couple of days ago." "Yes," he said, "they went through my back yard and they didn't even stop to say hello. I was at least going to go out and say, 'Come in and have a cup of coffee,' but they bulldozed my trees right down, right through my back yard." I looked out, and sure enough, that's what they did.

So we flew then up over to Anaktuvik Pass. We went out onto the ice toward Prudhoe Bay. We could see where the bulldozers were still working at the present time. Then we came back to Fairbanks and I started this series of conferences with different people--people who represented the Bureau of Reclamation, the people who represented the Fish and Wildlife Service, and the Bureau of Land Management. The Bureau of Land Management had a

big stake in it because they controlled a lot of land up there. We were appalled at how all these local people, they thought this was just fine.

Lage: All these people you conferred with were for it.

Leopold: Very few people were against it, and they didn't want to say if they did oppose it, because everything was for, you know, "We're going to get a lot of money out of this."

So I came back and I sat down and wrote a report that really told them where to step off. For example, I recommended--. I made certain specific recommendations: first, that certain parts of the pipe had to be elevated above the ground.

Lage: Because?

Leopold: Oil is hot, you know, and I didn't want it to go through the permafrost and melt the permafrost. I recommended that there would be a mile on either side of the road as a reserve with no hunting. I didn't want people shooting as I saw them doing there on the ground where they'd lean out of the truck and shoot things from the road that they were building. I wanted a stop on the killing of wolves, and several other recommendations of that kind.

So when the director read my report, he got pretty excited about it. This was a new director. This was when Pecora had just moved in, just before I quit.

Lage: Now, you say he got excited. Was it good excitement or bad excitement?

Leopold: He could see that this was a disaster.

Lage: It was hot.

Leopold: Yes. It turned out this just coincided with the Santa Barbara oil spill.

Lage: Good timing.

Leopold: As soon as the oil spill happened, there was Bob Curry at the University of California in Santa Barbara. He got all of his students together and they were out there walking the beaches making notes on everything that was happening. When the people finally woke up to the fact that this was serious for the tourist industry and that sort of thing, the only person who had any data was Bob Curry.

The secretary of the Interior decided he had to go take a look at this.

Lage: Now, this was Hickel, was it not?

Leopold: This was Hickel. The director, now, was Bill Pecora, just newly appointed director.

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Leopold: They got to Santa Barbara, and there were lots of things happening right then and there. First, no one seemed to know anything except Bob Curry. He was the only one that had any data, the only one that had really been out there looking. And then it turned out that all the people that were supposedly knowledgeable, they didn't even know where the oil was.

Well, apparently in the hearing that was held, Curry was saying effectively to the Geological Survey, to the director, "You people better get on this because this is an important geomorphic, geologic matter." He made the director very angry. I don't know how exactly it happened, but the director was very angry.

The director got back in town, and about that time my report was on his desk. It said, "Leopold, Skibitzke, and Curry." He looked at that and said, "Is that the same Curry I met in California?" "Yes, sir, it sure is." He said, "Well, take his goddamn name off of that, because I won't have it around."

Lage: You're kidding.

Leopold: I said, "What?" I said, "I wrote the report, but they were people on my team." I took the name off. The report then went from the director to the secretary, and then they began to see that it was very serious.

Lage: Was this a report of the same thoroughness as the Everglades?

Leopold: No, no, this was much shorter.

Lage: Much shorter. More of a recommendation?

Leopold: "For God's sake, don't give them a permit," is what I was saying.

Then at that time, because we were always causing trouble anyhow, the new director said, "You keep that Skibitzke out of that oil spill business." I told Herb, "You just hang on. The

time will come when your talents will be needed." Well, they couldn't find the oil. They had ships out there, great navy ships going back and forth, and they couldn't map the oil slick. Finally the director called and said, "Skibitzke says he can find that oil for me." I said, "You're damn right he can." He said, "Well, get him out there." [laughter]

So as usual, Herb had obtained one of these radar units, a big disk, you know, about six feet in diameter, sitting up on top of a truck. It was for tracking some kind of airplane. No one knew how to run this, but Herb took the thing apart, and he taught himself how to run it. He rewired the whole thing and got it running so that he then could pick up one of our little planes. So he rolled this damn machine out there to Santa Barbara and set it up on a cliff, and flew one of our light planes out there, and told my friend Howard Chapman, Chappie, who was one of our boatmen, "All right, you fly out there and you find that oil, and we are mapping it in this machine as the radar picks it up." As the airplane flew, a map of its course was automatically made in this trailer accompanying the radar disc.

So Chappie got out there. He called back and said, "Okay, I'm on the edge of the slick. Now I'm going to go flying around it. Now, you start mapping." So here was this great big slick, here came a little airplane. All those Navy ships down there couldn't find it. The little airplane flew all the way around.

Lage: He flew around the circumference of it? And then the radar traced where the--

Leopold: Yes. So in an hour we had a map of the whole thing, you see.

Lage: Herb sounds very clever.

Leopold: Oh, God, he's clever as hell.

So within an hour we had the whole thing mapped. That settled that matter. The survey had shown that we knew how to do things that nobody else could do.

Lage: How was the report on Alaska received?

Leopold: The Alaskan report was in the director's hands. He essentially told the secretary of the Interior, "You can't use that route and you can't build it. We're not going to give you a permit because you don't--"

Lage: So USGS had the right to give the permit?

Leopold: USGS had enough influence to say, "Look, here are the problems." They were quoting my report. Here are the things that they hadn't thought about. If you melt the ice, then what happens to the pipe? Does it move downhill? It will break. How fast is the ice going to be melted by this hot oil? What about the caribou? On and on and on.

Lage: In every area.

Leopold: Yes. Because those are all the things that I was talking about in my report. Well, anyhow, it stopped them. The Interior Department didn't give them a permit, so for five years the consortium had to start a big research program, which they did very intelligently. They set up a model pipe of the same diameter, a great big thing, up there at the University of Alaska at Fairbanks, and they put hot oil through the thing, measured the temperatures, and then they had a buried section, so they really learned a hell of a lot. So when they got through, they knew quite a lot about how to build that pipe so that it was worthwhile, but it cost them millions, of course, to be stopped. But it would have cost them millions if it hadn't been stopped, as a matter of fact.

Lage: And it would have cost more than that in damage.

Leopold: Yes.

Lage: Those are great tales.

Leopold: Yes, they are.

Lage: And important.

Recommendation on Redwoods National Park

Lage: What did you have to do with the Redwoods National Park? Did you make a report of the same type about that?

Leopold: No. When the secretary's office finally got onto the fact that the highest trees in the United States are in Redwood Creek, and the National Geographic Society wanted, what?--to buy it--? Or maybe--. Anyhow, there was something that came up about the National Geographic Society and the two big trees.

Lage: They funded a study, I think, and gave it publicity.

Leopold: It became quite clear that this was a serious matter, that the river was going to hell because the upper part, you see, is all cut over. There's nothing but this little strip of redwoods down the center of the creek, and all the rest of it is devastation. It's just devastated.

That was when Nat Reed was undersecretary (about 1974). He called a meeting here in San Francisco in which he asked various people to get up and discuss the whole matter of what do we know about what's going on, how serious are the floods, where is the sediment coming from, how fast is the river building up its bed, and what are the chances of destroying the trees, and on and on and on. Well, this went on for a day. I just was listening. I didn't know anything about it. I was learning a lot. Right at the end of the day I was sitting next to the secretary, and he turned to me and he said, "Very well, Luna, you summarize the whole thing and tell us what we ought to do."

Lage: That's quite a compliment.

Leopold: So I said, "All right, I will." I got up and I said, "Here's what you need to do. You have to set up gauging stations, you've got to make measurements of this and measurements of that." I said, "I'll even tell you the people who ought to do it. My first choice is Ed Helley, my second choice is Richard Janda." They're all survey people. They will do the job for you." The secretary said, "Fine."

So we immediately set up a program in which there were very competent people from the survey assigned to work with some of the National Park Service people, and over a period of several years they did a hell of a good job. They got all the measurements they needed and wrote a lot of reports, published the reports.

Lage: But you didn't have anything to do with--

Leopold: No, they wanted me to run it, you see, and I said, "No, I won't run it."

Lage: That was about the time you were leaving the survey.

Leopold: Yes. I said, "No, that's really not what I want to do. We've got lots of younger people that can do it just as well, and here are their names." The leader is one of the people that I recommended. Richard Janda was appointed by the secretary and did a very first-class job. My goodness, it took him--he must have worked five or six, seven years on it, and ran it very well, so the survey did itself proud on that job.

- Lage: It's a good example--and you mentioned five or six years--of how long these things take. It's not overnight.
- Leopold: No, you bet.
- Lage: This took place after the Redwood Park had been established but before it had been enlarged. In '78 it was enlarged. Did you have any thoughts about the wisdom of where it was established?
- Leopold: That was long past. In other words, we all knew at that time that the Sierra Club was really suckered into this thing. It was true we need a national park, but they would have, in my opinion--and a lot of people feel this way--they would have been far better to have said, "Give us a very small park consisting of virgin, uncut forest, and we'll stay with that," but because the big trees are out along the creek, the Sierra Club went for this little strip that has been called the worm, with all this devastation on either side, for the whole purpose of saving some of the big trees. In my opinion, it was a great mistake.

But anyhow, that's what we were stuck with, and therefore we couldn't do anything about that, but we could then start to talk about what's causing the problem, what are the dangers to the trees, and that's what the survey people did.

Scientists as Consultants on Environmental Issues

- Lage: Do you think overall the environmental movement has used scientists well?
- Leopold: Yes, in lots of respects they've used the scientists very well, but the scientific community simply is a poor match against the big money people, the developers and those groups. The environmental movement, after all, has very little money. They are beginning to get political clout, but not because they've got money but because they have standing.

This is one of the other things that's happening. There are an awful lot of people in this world, professional people, who are willing to sell their souls for money, and we see this all the time. Probably the best example that I know of is the delta of the Sacramento River. Several of my friends and I testified before Bay-Delta hearings of the California State Water Resources Control Board trying to point out to them what the dangers are. All these people, these big agriculturists in the central part of

the valley, have all kinds of money and all kinds of lawyers, and they have a lot of people who are willing to get up and swear under oath things that I think are simply clearly not true.

Lage: They hire scientists?

Leopold: They hire pseudo-scientists, not people with scientific reputations. They hire consultants, and there's the difference. Usually scientists stay away from consulting. The only consulting I ever do, except for one occasion which I got sort of caught in, are for places where I think that the environmental issue is so important that I have to get in and pitch. But ordinarily I'd just as soon not turn my scientific knowledge into-- . In a lot of cases consulting is very traumatic.

The Forest Service and the Denver Water Board

Leopold: So this past year I've been completely tied up in this big ruckus in Colorado. It's not fun, but we used our science very well. We started out with considerable disadvantage because of things that had been written prior to the time that we all got in it.

But yes, I think science is used very nicely, very well in environmental causes, but science doesn't take the place of money, really. It's not very often, for example, that you have the money to pay the consultants that went into this law case in Colorado.

Lage: This is the Denver water board case?

Leopold: Yes. The U.S. Forest Service requests the water court in Colorado to give the government a water right for instream flow in basins within the national forests of Water Division No. 1.

Lage: And who was paying you? The government?

Leopold: Yes. The Justice Department and the Forest Service. The Forest Service was putting up most of the money. The Justice Department was putting up the rest because the Justice Department was having to support its own lawyers. I think that most of our salaries as expert witnesses, came from the Forest Service.

Lage: Is there a whole group of you?

Leopold: Oh, yes. Most of them people I picked out. I was probably the one that pretty much laid out what we were going to try to do, and a lot of my friends were in it. One of my colleagues, David Dawdy, a former survey man who now is a consultant here in San Francisco, is a very important man in the case. We had some very good help from the Geological Survey. Dr. Richard Madole was our geologist, wonderful testimony. My friend Dave Rosgen, who was fired from the Forest Service and is now a consultant, was a very important person. One of my students, Dr. Ned Andrews, who now works for the Geological Survey, was extremely important, so all these people had to be paid.

Lage: Give me, just so we have this in the record, what the case is about.

Leopold: The case is about instream flow. It happened this way. About in the early 1980s, the Indian tribe on the east side of the Wind River decided that since a lot of water came into the Wind River from their reservation, they felt that that water belonged to them, because it originated on their reservation. So they asked that a water right be given to them for water which originated on their reservation.

At that time the Justice Department was involved in that case, and they went to the Forest Service. A friend of mine in Denver went to the Forest Service and said, "Look, if the Indians can do that, why in the world doesn't the Forest Service do it? Why don't you go and say, 'We don't want people diverting all the water out of the forest land, drying up all the rivers'? Why don't you ask for a water right?" Well, the Forest Service took them up on it.

So right at that time, one of the Justice Department lawyers and two of the Forest Service men, Lee Silvey and my friend Dave Rosgen, the two hydrologists for the Forest Service, were involved in trying to advise the Justice Department what the Forest Service ought to ask for and how to compute how much water they would like. Remember, now, they're not going to divert it. They asked for a water right to leave the water flowing in the stream on Forest Service land. Below the Forest Service boundary you can do whatever you want to. If you wanted to use it for urbanization, that's all right.

Lage: What area were they working on?

Leopold: This was Wyoming.

Lage: Still in Wyoming.

Leopold: Yes.

They settled out of court, and then several years later, the Supreme Court said we were right, and they gave the Indians a hell of a lot of water, because it was taken all the way to the Supreme Court. Well, then we knew that the situation in Colorado was not going to be so easy; they weren't going to settle, they were going to fight it. So the Forest Service asked the court, the water court in Greeley, Colorado, to give them a water right; they applied for a water right through the water court, which is according to the state rules. And then the other side, the developers, particularly the attorney general of Colorado and the Denver Water Board and a whole lot of little irrigation districts on that side of the mountain, they all got together to fight it.

Lage: Because they had been using water that originated in--

Leopold: No, they were afraid that they would be prevented from taking additional water out of the forest. They were already taking a lot, but they were talking about the future. And of course, we were saying, "We're not preventing you from developing. Our argument is, we want only enough water to see that the streams don't go dry, that the streams maintain themselves." Our computations ended up by showing that if the streams were allowed to keep half of the water divided in a certain way, that that would be sufficient to keep the streams operating as usual. But the other side is not interested in half. They want all. They want to dry it up. They really want to dry up every stream on the mountain. That's what we were fighting.

We were in very bad shape because the Forest Service years ago, soon after the Wind River case in the early eighties, had already made a claim, and now we came in here ten or more years later, and we said, "We don't like the way that was computed." So three times since the beginning, the Forest Service claim was changed. The last was changed two weeks before the court case ended. The judge said, "That's enough. I won't allow that." But we were stuck with things that had been done earlier. The state said--

Lage: Before you came on the case?

Leopold: Yes. Things that were done before we had anything to do with it. I think that we would have been better off if Justice had decided, "No, even though that may be better from the standpoint of you hydrologists, from the standpoint of winning this court case, we'd better not ask for any more change." But they did. I didn't have anything to do with that decision. I had a lot to do with what was going to be recommended, but the decision was made

by the Justice Department as to what they thought they could do. So it set us back a lot, and we don't know how the outcome's going to be.

Lage: Is it in front of the court now?

Leopold: Yes. We don't get an opinion until next December.

Lage: But the work on it is finished. You're just waiting.

Leopold: And I'm sure it will be appealed. It's going to go to the Supreme Court.

Lage: It would set quite a precedent in many, many places.

Leopold: That's the reason we're fighting, because we'll have the same problem in--.

The Forest Service has made some tremendous mistakes in this. The most important mistake that hurt us the most was in a case in southern New Mexico on the Mimbres River. There were objections to the Forest Service asking for water, and it was taken to the court. The Forest Service really didn't call in experts and say, "This is an important matter. We'd better do this carefully." They got licked. The Supreme Court said this: "We think you deserve some water, but you cannot claim it for anything other than what was written by the Congress in the time the Forest Service was formed in 1897. You cannot claim Fish and Wildlife, you cannot claim recreation, you cannot claim aesthetics, you cannot claim anything else" except the two things that the Congress said, that "We will set up the Forest Service reserves for two things: one, to grow timber, and two, 'for favorable conditions of streamflow.'"

Lage: Well, at least that was in there.

Leopold: All right, but the whole court case hangs on what those words mean. Everything hung on those words, because we couldn't use anything else. So we were trying to prove from the geomorphic standpoint what the Congress must have meant.

Lage: Even though so many things have intervened since then that give the Forest Service jurisdiction or protection.

Leopold: The Supreme Court said, "No, you can't claim it for any other purpose than the original purpose of the Forest Service as stated by Congress, because we were asking for a priority date of 1897.

Lage: Oh, I see.

Leopold: We were asking for a priority date. Now, one of the things that I recommended, and I don't think that they're doing, is I said, "Look, if we don't win this case, go in and claim a 1990 date. Go in and ask for the same thing with a 1990 date." I don't think they've done that. We have already decided not to impinge on any present rights. We're only talking about future rights.

Lage: So you'd asked for the amount of water that they have now.

Leopold: Yes. We were not asking for what they have now. We were asking for the stuff that they haven't yet used.

So as I say, there have been a lot of things that have been decided but have not boded well for us. But it's very early to tell. I haven't any idea of what's going to come up.

Lage: Did you spend a lot of time on this? Has this been a major commitment?

Leopold: Oh, yes, a lot of time.

A Turbulent Time on the Sierra Club Board of Directors. 1968-1971##

Lage: I wanted to talk about the Sierra Club and your service on the board of directors.

Leopold: That's not a very important story, actually.

Lage: I've done a lot of interviewing of Sierra Club people, and you brought kind of an outside perspective to the board and to the turbulent time in the club that you were plucked down into. How did you happen to run for the board of directors?

Leopold: Because one of my friends in the UC Department of Geography had been very active in the business. He said, "I wonder if you would be willing to run for the board of directors?"

Lage: Who was this?

Leopold: It was Dan Luten. I said, "Sure." I ran for the board never thinking I was going to get on it. Well, anyhow, I got on the board.

Lage: It wasn't Dave Brower, then, who asked you?

Leopold: No.

Lage: So this was 1968--from '68 to '71.

Leopold: Yes. There was plenty to argue about, because Dave had greatly expanded the publication business, especially overseas. His original idea of having these coffee-table books turned out at first to be very successful, but then he had other ideas that I can't remember in detail that were expensive ideas that just didn't seem like they were going to be as successful. The financial way in which the Sierra Club was going was very troublesome, and Dave was essentially embarking on some rather questionable ventures.

Lage: In the financial field?

Leopold: In the financial field, yes. Primarily in publishing. Then there was the other thing. It appeared to people who had worked with him over a long period of time that he tended to pretty much do things his own way, even when the board was essentially warning him not to do it. So even the people that admired Dave a lot, like Ansel Adams, were just tired of having the financial thing so really out of control.

Lage: You came in at the point where Brower's opposition was starting to bring charges, formally.

Leopold: Yes. Right at the time that they were basically bringing charges against him.

Well, when the thing finally came down--. After lots of discussion, the thing finally came down to a vote, and only three of us voted on Brower's side.

Lage: This was after the May 1969 election [when Brower and a supporting slate of candidates ran for the board of directors and were defeated by an anti-Brower slate].

Leopold: This was when the question was, were they going to fire him, is really what it amounted to. In effect, that's what it amounted to. Eliot Porter and Martin Litton and I were standing up for Dave. Phil Berry and Ansel and Dr.--

Lage: --Wayburn.

Leopold: --Dr. Wayburn and Will Siri as well as a couple of other people were on the other side.

Lage: How did you make your choice?

Leopold: I felt that the thing that the conservation movement needed was to keep this very flamboyant, well-recognized man as our spokesman. I felt that we should be able to find some other way to curb the problems that we were having with money in order to maintain the charismatic leadership that Dave was furnishing to us. Furnishing to the whole movement. I still say I would have voted the same way. I'd still feel that, although I think that Dave got even more difficult to work with, because, you see, they had this breakdown in Friends of the Earth, so it is true that Dave is a person that people find very hard to get along with. I don't know the details of what happened at Friends of the Earth, but the fact that it happened again indicates that something similar must have been going on.

Lage: After he left, you were still on board for a couple of years.

Leopold: Yes.

Lage: Did you see a change of direction or a great loss in any particular way? Phil Berry became president.

Leopold: Let me give you an example. Dave felt that the Sierra Club should be completely against the Diablo Canyon nuclear plant, and I definitely agreed with that, for a lot of different reasons. But I got in a big argument with one of the persons on the board about the question of what would happen to the cooling water. The argument that I was given was, well, the water will come out hot, but it will be good for the fish. I said it may be good for increasing the number of fish, but if you start changing the character of the ecology of the coast, I would say that's really not the way that the Sierra Club ought to go.

Then the fact that we weren't just talking about Diablo Canyon, we were talking about cutting these swathes across the whole Coast Range for the transmission lines, that's one example of the place that I supported Dave in this, and most people were on the other side.

Lage: Were you concerned about nuclear power at that point? Or not?

Leopold: I don't think anybody was as worried about nuclear power as the question of disposal of wastes and the question of water. That's where the big argument was.

Lage: And the scar on the land.

Leopold: Yes.

Lage: Fred Eissler, if you remember him, was on the board. I think he was the only one who was bringing up the question of the safety of nuclear power per se, but I just wondered if you'd had a--

Leopold: I don't remember that. No. I remember that at a meeting in Greece of the International Association for the Conservation of Nature and Natural Resources in 1958, there was introduced a motion saying that what we ought to do is to go for--how was it first stated? It was a question of hydroelectric power versus nuclear power. I said, "Gee, you don't seem to understand that there are great difficulties with each of these two things. For goodness sakes, don't land on one as better than the other." Anyhow, I cut that one down to size.

But there were a lot of things that had not yet surfaced at that time. The whole question of nuclear accidents was not the thing that was the most important. My recollection is the things that were primarily the problem was disposal of waste and the question of water and also the whole question of earthquakes, which was why they stopped the nuclear power plant at Bodega Bay.

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Lage: Did you get to know Martin Litton on the Sierra Club board, or had you known him before?

Leopold: I hadn't known him before, but I've been down the Colorado with him since, so that I've always been an admirer of Martin.

Lage: He's a pilot also.

Leopold: Yes, indeed, he is. As a matter of fact, when he left Sunset Magazine, he unfortunately had to leave his airplane.

Lage: He seems like a man who will always find a way.

Anyone else on the board that you'd want to comment on, or qualities about the club?

Leopold: I've worked with Phil Berry since then very closely. I'm a great admirer of Phil's.

Lage: On what did you work with him?

Leopold: I was one of his expert witnesses on a trial that we had a couple of year ago down in Orange County, where we tried to save the islands on Newport Bay. I didn't know Phil very well when we were on the board together, but much later, when Phil asked me to

join him on this law case and I pitched in and did it, I became very fond of Phil. I had never met Michele [Perrault, Phil Berry's wife and also a former president of the Sierra Club] before that law case either, but I'm a very great supporter of both of them.

Lage: Was this a recent law case? Because when he was president, there was a law case he got involved in in Newport Bay.

Leopold: That was it.

Lage: But that was way back at the time you were on the board.

Leopold: No, no. No, that's another one then. This is one about five years ago. We enlisted a lot of university people on our side. It's my personal opinion that no one could win a lawsuit against the Irvine Company in Orange County. Just the way the judge ruled and the words that were used in the ruling make me think that we didn't get a fair trial at all.

Lage: So it wasn't a successful suit?

Leopold: No, we lost the suit.

Lage: During the time you were on the Sierra Club board, the club, or at least the club president, took stands and testified against the nomination of Secretary Hickel and then Rogers Morton, for secretary of Interior. Would you have gotten any flak in the USGS about membership on the Sierra Club board?

Leopold: Yes. This comes up again and again. I don't know of any law cases I was ever in but what they brought it up.

Lage: In law cases they bring it up as if this were a disqualifying thing?

Leopold: Sure. Oh, you bet. No, I can think of at least two--. I'm sure I'm right about this. Yes, I have a very definite recollection that this is brought up against my record, that you have been connected with the Sierra Club. I said, "Sure, I've been connected with the Sierra Club. I'm very glad to do it." My testimony and my opinions have mostly been shaped by my scientific work. Yes, that has not been a useful qualification in some of these things where we have really big fights about conservation.

Lage: I see. Because you're being presented as a scientific witness, an expert witness.

Leopold: Yes. And of course, they're trying to make us sound biased, you see. I said, "You don't have to be biased to be a conservationist." As a matter of fact, one wonders how anybody can know anything about science and not become a conservationist in trying to protect some of these things that are under siege. In other words, I never tried to make out that there was anything wrong with that. One wonders how you can deal with scientific matters and not see the need for the protection of some of these natural values.

Lage: Did the USGS object to your being a member of the Sierra Club board of directors?

Leopold: It was never brought up for discussion as far as I know.

Importance of Aesthetic Values: Hells Canyon

Lage: You'd also been part of the Wilderness Conference presentations before your board service. Were those something that stand out in your mind at all? The Sierra Club Wilderness Conferences?

Leopold: Well, I gave papers there. I haven't read those papers for a long time. I can't even remember. One was called "The Dragon to Slay."

Lage: Right. It seemed like one of the themes--maybe in both of them that I read--had to do with the importance of the non-monetary aspect of wilderness.

Leopold: Well, of course, that's what I worked a lot on. That's been a very important part of what I accomplished, I think.

Lage: Your paper on Hells Canyon [of the Snake River], is that an outgrowth of this concern with the aesthetic, non-economic values?

Leopold: Yes. ["Quantitative Comparison of Aesthetic Factors among Rivers" (USGS Circular Series, 1969).]

Lage: How did that come about? You did that while you were with USGS. Did that come about in response to the effort to save Hells Canyon?

Leopold: It came about because there was a hearing before the Federal Power Commission on whether they were going to grant a license for another dam in Hells Canyon. Alan Kneese was working for

Resources for the Future. He and another well-known economist were going to give testimony before the committee on the economics of recreation. In some manner or another, Alan Kneese got me interested in looking at other aspects of it. I said, "It seems to me that one of the things that is most important is to recognize that they are undervaluing the aesthetic aspect of the canyon in its original condition." So I made a trip to Idaho and learned a lot about Hells Canyon. I wrote this paper on how one might compare different scenic areas. That made quite a splash but I don't think we won the case at that time.

Lage: You talked about quantifying the aesthetic features.

Leopold: Yes. It was a way of trying to deal with non-quantifiable things to put them into forms that people could see.

Lage: Did that get taken up at all, do you know, say, in making EIR [environmental impact review] statements?

Leopold: It was certainly taken up in Canada. I was invited to go to Winnipeg. I was teaching for a semester at the University of Ottawa, and the Canadian Park Service asked me to go with them to Winnipeg and talk to them about how they might use my scheme in the choosing of areas to be protected as wild rivers and national parks. So they probably did more with it than anybody here has done with it. I heard later that indeed one of the places that Herb and I flew to was, because of the scheme, turned into a national park.

Lage: That's gratifying.

Leopold: Yes.

X LEAVING THE GEOLOGICAL SURVEY FOR THE UNIVERSITY OF CALIFORNIA

[Interview 7: March 12, 1991]##

Resigning as Chief Hydrologist and Subsequent Changes in the Division

Lage: You mentioned earlier that when you left the position of chief of the Water Resources Division, they made a lot of changes.

Leopold: Yes.

Lage: Did they change your policy of sending people to school?

Leopold: Yes. For a long time there was nobody sent to school after that. I don't know. They kept me so far away from it that I really couldn't tell you very much about what the operation was.

Lage: So when you left the directorship and became senior hydrologist [1966], you were no longer involved in the management arena?

Leopold: They prohibited people from talking to me.

Lage: Tell me about that. We haven't recorded anything about that.

Leopold: They appointed my assistant chief as chief.

Lage: You decided yourself to resign?

Leopold: Yes, well, that happened this way. After I had been in the chief's job for some time, I went to a man in the Geologic Division whose name was Dr. Pecora, who was a well-thought of geologist in the Geologic Division. We had been talking about the survey, and I said to him, "What do you think? In your experience with the Geologic Division where you trade people, how long should a man keep his job?" He said, "I'd say five years because that's what we do in the Geologic Division." He said,

"On the whole, we trade people in and out of the administrative jobs about every five years."

I'd been in this job for ten years, and now this man, Pecora, was just made the director. I went to him and said, "Do you remember that some years ago, you and I had a discussion about this matter about trading? I think that we need some new thoughts and some new ideas. At that time you suggested to me that five years is what you did in the Geologic Division. I've been here ten, and I think it's time for me to move." He said, "Yes, I think that's a good idea." But the people that took over--

Lage: Who was appointed to replace you?

Leopold: The man who was my assistant, Roy Hendricks. I had not recommended him for that job, but anyhow, that's what the director decided. The main difference was, or the thing that precipitated it-- Well, there were a lot of other things, too. I was very unhappy at home and was to get a divorce after a short time, so I wanted a change anyhow.

But right at that time, I started to enforce for the first time, against anybody's will, the idea that I wanted people to move. There was a man in my office that I wanted to move. I'd set up good jobs for these people. He's the first one that said, "No, I won't do it." It had always been the custom in the survey that when they asked you to move, you moved. But on the other hand, there was also the custom in the survey that they treated you very well, that the move was always to your advantage, not to your disadvantage, but the point is that you were moved at the discretion of the organization.

There was some kind of a pressure being brought in the director's office against this decision that I'd made. I'd worked this out very carefully with the assistant director. Well, it was at a GSA [Geological Society of America] meeting, and I happened to bump into the new director. He pulled me aside and said, "Hey, what the heck's going on? I hear all this trouble that you're causing." I said, "What trouble?" He said, "You're trying to transfer Mr. So-and-so and he didn't want to be transferred, and they're objecting to my office that you shouldn't do this." In effect, he was saying, "You're embarrassing me."

I said, "Director, that isn't true. I've been working for months on this problem, and I have it all straightened out with your assistant director. I have his approval. It's been worked

out with your office over a long period of time." "Oh," he said, "I didn't know that." I said, "That's the way it is."

Well, right after I left my job, it was decided that the man need not move. From that time on no one has ever been moved against his will. It changed the whole character of the organization. It made a lot of difference because people could say, "You didn't move him. He objected, and you didn't move him."

Lage: Were there other aspects of your policies that were rescinded?

Leopold: Well, you see, anybody who is trying to do things in a new way-- and we've spoken before about the fact that they thought I gave too much attention to research and not enough attention to basic data, so I made a lot of people unhappy. The man who took my place came up through that line of work, you see. He had all these friends in the field, and therefore all these things that I was doing were now considered to be the wrong things to do because I had paid too much attention to the young research people and not enough attention to the old guard. So it changed radically.

Lage: You said that they sort of kept you separate.

Leopold: People in the organization were advised not to talk to me. In something like four years in the Washington office, only one person walked in my office in four years. They were enforcing it. They didn't want anybody to talk to me.

Lage: It gave you more time for your work.

Leopold: Yes, but the point is, you see, what they were afraid of is that I was going to mess in their business, which I had no intention of doing, but that's what they were fearful of, apparently. Anyhow, the word went around that that was not acceptable; they couldn't talk to me.

Anyhow, a lot of things might have been done differently. One would suppose that the new administrators might seek the advice of more experienced people, but the idea that you were actually prevented from saying anything was really too bad, I thought.

Lage: I would think so. Would you say that certain of your programs have survived? Surely the ten years that you were there must have made a difference in the organization.

Leopold: Oh, it changed everything. Now people look back at it, you see. "Those were the halcyon days of when things were really going."

Many things that I started that they didn't like at the time now have been expanded greatly. For example, I finally decided I was going to hire one biologist. There was not biologist in our whole division of 3,000 people. Now they have forty biologists. Many of the programs I started they found were darn good programs that they've expanded all over the place.

Lage: And just the whole building up of the research division certainly didn't change.

Leopold: Oh, yes. They never added to that, in fact, but that's one of the big problems they've got now, is that they've not added to it except very incrementally, in very small amounts. I was told a short time ago, within the last year, that the whole research organization is still running on the money that I got the first year, which is twenty-five years ago. With everything else expanding, you'd suppose that they'd have expanded that, but they never have.

Problems of Maintaining Productivity in a Research Staff

Leopold: And now many of the people I hired are now not very productive because they're of such an age that production goes down, of course, but they won't be moved. In other words, once they didn't do what I said when we were going to move people--and I was putting myself in the same position--now they're stuck with overage people, and they don't know what to do with them. As a matter of fact, one person that I hired, I saw some years ago, not very many years ago. I happened to drop into his office--I forget where he was--and I said to him, "What do you do with your time?" He said, "I come to the office to draw my pay. I sit here and carve with my pen knife on a piece of wood. That's what I do." I said, "You're serious?" He said, "Yes, I'll show you."

Lage: He sounds very bitter--or very cynical.

Leopold: Both, I think. I don't know. I think he was one of the kind of people that I thought was doing very interesting work that was different than most people did, but I suspect that he was not much appreciated by the new outfit.

About four or five years ago, I guess it was--this is now twenty-odd years later--I was asked by one of the research men to come to Denver and give a talk to all the research people for all

the western states, which I did. There were a lot of people that didn't care much for what I was saying, either.

Lage: What was your talk about? The survey?

Leopold: I was saying about the research organization, I said, "The main problem that you've got now is that you have a bunch of people that are overage, and they're not as productive as they used to be. You ought to find some way to change it. I suggest you do the following. First, set up a senior--I'm not sure what to call it. Send them to school. Send the senior people back to school. Let them come to universities and get retreaded with new ideas. Meet some younger people, meet some different people, get the heck out of the office. That's going to take money, but get hold of some travel funds and send people back to school."

Lage: Were you suggesting this for the research people in particular?

Leopold: Yes. I said, "These people now, that I hired," and I was talking to many of them, I said, "you guys are just overaged. What you need is some refurbishing. In the university, boy, you'd get refurbished in a hurry because you meet young people with lots of ideas, people who want to do something, and they will keep you on the ball. I think that what would do us a great deal of good would be to give our people a chance to go back to school."

Then there are other problems: the problem of promotion, the problem of direction, the problem of judging scientific productivity. I think that improvements could be made in all of those things.

They didn't go for these ideas, but last year I was at a meeting, and I met one of the young people who was in the research organization and moved up to being kind of a supervisor. He was complaining to me about how things used to be and how things were now. Now, this was a younger man who actually worked at one time under Tom Maddock. I said to him, "I think you've already now been in administration too long. You went from research into administration, you've been there, you've tried to do a good job. You need refurbishing. I'll tell you, you come to Berkeley. You come and share my office with me. All you have to do is sit and go to the library and think and write and do what you want. You don't have to work for credit, you don't have to try for a degree, you don't have to do anything. Just come and meet some of the young people that come through the university, and sort of get some new ideas."

Well, nothing happened with that, but later last year, in the fall, the same problem came up with the Forest Service in

connection with a court case. I was dealing with all these Forest Service people and I could see that--. Because, you see, I'd been giving courses for Forest Service people last year. I gave eight courses last year.

Lage: For hydrologists in the Forest Service?

Leopold: For trying to give some up-to-date hydrology or some detailed hydrology to Forest Service officers.

They had just appointed a new hydrologist--the regional hydrologist in the Denver region. His name was Jim Maxwell, and he was very impressive young man. We were talking about the fact that the Forest Service had run out of hydrologists, and we were now trying to bring it up to date with the courses that we were giving, and I said the same thing to him.

I said, "Say, Jim, why don't you do this? Why don't you promote in your organization what I suggested to the Geological Survey? Set up a sabbatical leave." He said, "That's a wonderful idea." I said, "I will assure you that if you do so, I can get you places at Johns Hopkins, at the University of Washington, the University of Arizona, Berkeley, and possibly other places. We would be delighted, and I would be delighted to make arrangements for you to have your people welcomed in the office of some hydrologist in one of the good universities where you'd have no responsibilities except to just participate as you wish, to write and think and read." So it may turn out that maybe somebody will do something about it.

There are two parallel problems that have to be addressed by a supervisor in a game of this kind. First, as people get older, they lose the sort of intuitive ability to keep going at a rapid pace. Secondly, there's so much literature to read that unless there's some very specific way that people have seminars where they have students, they have weekly meetings, they trade ideas, they soon are not following the science. Those two things are parallel and they have to be attacked simultaneously. In fact, so rapidly does the literature grow that no one can really keep up with the literature of the whole field that you might be interested in. Therefore a lot of it has to be seeing people and talking to people and finding out what other people do, which will stimulate you to do things.

Deficiencies in University Reviews for Promotion and Ph.D.'s

Lage: Has there been a tendency towards more specialization all along, in order to cope with all this literature?

Leopold: Yes. I would say much more subtle and much more serious is that we have now in science in general, at the universities, in university departments, in research units, the idea, the crazy idea, that the way you get ahead in this game is to write a lot of short papers so that you actually have--you're just counting numbers of things you've published--not the content, but how many papers you published.

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Leopold: Now, this is serious because, in fact, the way promotion is going on nowadays in university departments--I can tell you in the departments I know in this university, as well as in government agencies, they are paying attention primarily to how many papers, not what the paper says. Now, this simply has to be reversed if we're going to maintain our ability to do science. Far better, there are several ways we can do it.

One, when a person comes up for promotion, the thing I would recommend is that the nominee should pick out for the visiting committee or the committee who's judging him three to five papers that he considers the most important work he's done. Let them judge on that, but do not let them see the great number of papers one paragraph long that were published in a fancy journal.

Lage: Because do you think they only look at the titles and don't review the papers themselves?

Leopold: Exactly. I can assure you they don't. I can assure you that's so.

Lage: It's such an important process, this review of people--

Leopold: I can tell you that that's what happens. As a matter of fact, there are good reasons to believe that supervising professors are not really making a real study of the Ph.D. theses that their students produce, that they kind of take an overall look at it, but not the same kind of careful review they would make if they were working in that subject themselves.

I think that that's a very harsh thing to say, but I think it's true, even, as I say, when a supervising professor is not

actually understanding the details of what a student of his is saying. That's a pretty serious matter.

As a matter of fact, this is a procedure that I'm describing so prevalent that the young people talk about it. "Oh, well, the way to get ahead is I'm going to write a lot of papers, even if they're only one paragraph long."

Lage: It's a real careerism attitude, instead of a professional attitude.

When you were chair of the Geology Department here at Berkeley, did you try to address this? Or at other times, have you tried to address it here?

Leopold: Yes, there were a lot of times. Yes, there were things that happened that were so unbelievable that-- That's another whole yarn.

Lage: Shall we talk about that now?

Leopold: I was on a Ph.D. committee in another department; I was one of the outside examiners. This person was in the middle of the examination. It turned out to be a field in the field of water.

Lage: So this was an oral exam?

Leopold: An oral exam. This was a Ph.D. oral. I said, "This proposal that you're making to do this research, how much water is involved?" The student said, "I don't know." I said, "Can you make some kind of a guess?" "No." I said, "How would you measure the amount of water?" "I don't know that." I said, "You're writing a paper on water? We're talking about amounts of water, and you don't even know what the units are?" I said, "You flunk." The chairman of the department, at the end of the examination, turned to me and said, "Well, Luna, you're going to keep us honest, aren't you?" I said, "You're goddamned right." At this university.

Lage: Did you sense resentment on the part of the chairman of the department? Would they have let that go by?

Leopold: They would let it go by. As a matter of fact, that student took another two years, I think, to finally change thesis subjects and do something else. So it goes.

Then there was another Ph.D. oral, same kind of a problem. Anyhow, I'm absolutely convinced that there are many real holes in the way students are being handled at the universities and the

way research is being handled throughout the whole system. I think it's a very serious matter.

Lage: Is it based on this sort of careerist attitude?

Leopold: I'm not sure that's the right word.

Lage: What word could we use here?

Leopold: Lack of objectivity on the part of supervisors, and peer review. Lack of objectivity. There's an awful lot of personal chumminess that goes on in these matters--promotions, for example, in government agencies. Now, in universities it's being done correctly and incorrectly. It depends upon where you are. The best systems for promotion that I know were the systems that I experienced that were going on at Harvard. In my opinion, they're far better than what I find here at Berkeley.

For example, there was a time when a friend of mine at Harvard was up for promotion. This was John Miller. One of the most famous geologists in the world came to see me in Washington and said, "I am on the committee that has to do with the promotion for John Miller, whom you know." I said, "Yes, John Miller and I worked together very closely." He said, "We're having a hard time because we're not sure, on your joint papers, how much he did and how much you did." I said, "Let me try to explain to you." I did my best to explain what each of us did, what our contributions were, for several of the papers we'd written together. Then they turned him down, temporarily, you see.

So next time I saw John, I said, "John, you and I have to stop working together. We'll have a lot of conversation, but we're not going to work together and publish papers together because you have to do some of these things on your own and prove to these people that indeed your contribution is certainly equal to my own, and that's what we're going to do."

After several years I had another caller, and this time who was it but McGeorge Bundy, the famous man who at that time, before he was on the president's staff during the Vietnam War, was the dean of graduate studies at Harvard. He came to see me. He said, "We have a proposition for promotion for John Miller." I said, "Let me explain to you what happened in the past. For the past four years, as you know, we haven't worked together so that he could prove to all you people that he really is a man of great competence in his own right." He was immediately promoted, but that kind of care that Harvard was taking in this particular

case that I know about was in my opinion far different from what I see go on here.

Lage: When you read about the process here, it sounds extremely careful. For instance, I was just reading the controversy about the woman mathematician. I don't know if you've heard of that.

Leopold: No, I don't know.

Lage: There was a recent article about a woman in the mathematics department who was not promoted. Hers, in fact, was a case of working on what's considered an important problem, but she had one important problem. Anyway, as they described the process, it sounded very careful. You know, the ad hoc committee that reviews it, and then the department chairman, and then the College of Letters and Science dean, and then it goes to the Academic Senate Budget Committee.

Leopold: Yes, that sounds great, but there's only one level at which the actual production of the person is actually reviewed.

Lage: Where they actually look at the work. And which level is that?

Leopold: That's the level of the ad hoc committee. And then you're not sure how much care the ad hoc committee took itself. When I was in the Department of Landscape Architecture one of the things that I was very firm on--and they never followed my advice--was that I suggested to them that since many of the people in our department were coming up for promotion to an ad hoc committee made up of scientists, that their artistic work was not judged properly because they say, "This is a piece of art," despite the fact that it says artistic production, music, poetry, all these things, they all count. But the fact is that so many ad hoc committees on this campus are made up of people that come from the sciences that they look at these things and say, "How am I supposed to judge this?"

Lage: You mean a committee with scientists would be judging landscape architecture?

Leopold: Yes. So I suggested that we all work together to prepare a kind of a model to show how to present a piece of artistic work, original artistic work, to a committee that has really no way of judging it. I saw one man turned down; I saw what they had presented to the committee. It had been reduced in size from some large panel to something that was page size. You couldn't read the doggone thing. It wasn't in the proper color. It was poorly prepared, despite the fact that his work might have been good.

Well, I thought that was just plain-- Well, it was inexcusable, because the fact is that the artist ought to be the one who knows how to present things so that you really make an impression, so I wanted a sort of a model in that department. Well, they never did it. And there were a lot of people that were turned down because they didn't make the grade in the ad hoc committee.

Lage: Have you served on the ad hoc committees yourself?

Leopold: Oh, yes, indeed, I certainly have. Yes, many times.

Lage: For people in various departments. Is it hard to judge the work? They wouldn't put you judging a mathematician, surely.

Leopold: No. Nor would I accept it if they did. But things that I can judge, yes. For example, forestry, soils, engineering, landscape architecture, geology--I've served on all of those committees. And physics.

Lage: So was it from these experiences that you felt that perhaps the other members of the committee weren't thorough enough?

Leopold: Yes. One is lucky if there's one man on the committee that really sits down and does the homework.

Lage: Is doing the homework most often to the individual's advantage or his disadvantage? Are you more often to find fault or to--

Leopold: To his advantage. Very definitely to his advantage. No, I would say without question to his advantage.

Trend toward Unimportant Problems and Short Research Papers

Leopold: We've gotten onto a subject that goes to the totality of one's experience dealing with scientific matters and organizations. The other thing I want to say about this whole field is that I find that the people that I know, many of the people I know, are concerning themselves with problems that I consider so small and so relatively unimportant that it's neither worth their time nor the department's time. I think I've said to you that I've urged people, both when I was in the Geological Survey and when I was here, that I suggest to all people who are in science that they should keep in their private file a little folder called "idea file" which deals with their ideas of what are the most important

problems in their science. I said, "Look at it once in a while. Here you've thought about these as really important problems. Are you the right one to do it? Can you do it? If you do it, it's worth it."

Secondly, the Geological Survey people fall in the same trap that the university people are in, and for the same reason. They're led that way by the promotion route. What the people look at is how many papers? Not what the papers said. The idea of writing a thick tome on something that you've spent ten years on, people nowadays say, "That's not worth it to me. I can't get promoted on that," when the fact is that the detailed work usually turns out to be much more significant than just a whole series of short papers. Much more.

Lage: When you say "short papers," you're really talking short--half a page?

Leopold: I'm saying that I've heard the younger people in this department say, "The way to get ahead is to write a paper that's less than one page long, and publish it in Nature, and do it five times a year. That will get you ahead."

So the idea of writing a book-- Well, there hasn't been a book written in this department since [Howel] Williams and [Ian] Carmichael, and [Charles] Gilbert on petrography [1958]. The [John] Verhoogen book was quite some time ago; my book. The idea of compiling something of some detail.

And of course, the book is not necessarily the way to deal with new ideas. It's really the difference between compiling a lot of things and striking out on absolutely new ground. So forget about books for a minute, but I'm talking about papers prepared over a long period of time that result in a Ph.D. thesis. For example, I will warrant you that most people that I know have never and probably will never write anything as detailed as their Ph.D. thesis. But after they get their Ph.D., then "We're going to go to the short paper stuff, and we can't waste three or four years on anything."

Lage: Does the Ph.D. thesis get published in an article form?

Leopold: As a matter of fact, to my knowledge mine was the first one that ever was done that way. The professors at Harvard didn't like it at all, but I said to my professor, "Look, I'm dealing with four different subjects, very different, and I would suggest that I publish this as separate papers and that each paper is written for a particular journal." He said, "That's fine. Let's just do

it." Then I heard later that the rest of the department didn't like that at all.

Lage: The idea of breaking it into four papers?

Leopold: Or five or whatever it was. They wanted the same old thing where you wrote a great tome and everything was tied together. I have the same problem with one of my Ph.D. students here. One of the other people on her committee, in another department, my lord, went back to the old idea that you have to spend the first year on studying the bibliography, and then your thesis has to start with a review of the literature, and then you go on to the method of research, and then you go on and on and on--things that were outmoded fifty years ago. That's really not the way modern science is done.

Lage: Why do you suppose her professor brought that up?

Leopold: I don't know. I said to the student, "Okay, if that's what is required, you just do it, but keep in mind, now, that you're going to break the thing down into the units that you can publish separately. All you want to do is get your degree. I don't care how you do it. Get your degree. This is a bunch of nonsense." The thesis itself was one of the longest theses I ever read, and the reason it was so darn long was that another professor had forced that student to really make one great big tome out of it when it should have been, in my opinion, divided up into units that were logically publishable. But these long tomes--which is another problem in science.

In the Geological Survey, as I've told you, in some of my publication policies, the one thing that I guaranteed was that people could publish what they wrote with a minimum of careful review, and that there would be no limit on length. Now, the way science is going now, there aren't very many places in the world you can publish a long paper anymore. I'm told that even the Geological Survey now is getting unhappy about publishing papers that I used to think were the right size--fifty or a hundred printed pages.

Lage: Is this a cost consideration?

Leopold: That's partly cost, yes. Partly cost. For example, you hear again and again of important journals that will send a manuscript back to the author and say, "This is an interesting paper, but cut it to two-thirds of its length, or cut it in half." Well, you can't cut a paper in half and still have what you originally sent in.

- Lage: You also don't give the reader quite as much background on how you came to your conclusions and your methods of research.
- Leopold: Another thing that is very hard to do now, which I insisted on doing in the Geological Survey, is publish your data in tabular form. I said to my people, "What you say is going to run out of date, but the numbers that you produce will never be out of date. They can always be reanalyzed by somebody. The ideas that you got from your numbers may turn out to be subject to interpretation by somebody else later, but your numbers won't be. You try putting a long series of tables into modern scientific journals, and they're simply not going to take them. There are very few journals, very few journals indeed, who will publish detailed tabulated data. The American Philosophical Society is the only journal that I know which, if you're a member, will publish what you write, period.
- Lage: Of any length?
- Leopold: Yes.
- Lage: Well, they must review it in some way.
- Leopold: I think it will be only an editorial review because there are only five hundred members, for goodness sakes, in the whole society.
- Lage: You've already passed muster.
- Leopold: Yes, you've passed muster. By the time that you get there, you already know something about publication.

Encouraging Careful Acknowledgment of Ideas

- Lage: Do you see a problem with honesty in research techniques in publication?
- Leopold: Only in this regard. No, not in publications. No, the thing that bothers me the most is the matter of acknowledgement. I say to my students all the time, "You will never hurt yourself by being extravagantly thankful for the people that helped you, and you'll kill yourself if you don't."
- Lage: Acknowledging who influenced your ideas?
- Leopold: Now I say to people, "In science, for goodness sakes, we're always building on somebody." After all, we don't acknowledge

Mr. Newton anymore but you use the Newtonian thinking no matter what you do, so that you're building always on somebody else's shoulders, and everybody knows that.

A recent example. One of the people in the Geological Survey that I hired quite a few years ago, presumably because of jealousy published a paper in which he was compiling some material from quite a few authors. Of the many things that I've published, he chose that one portion of our data that didn't have my name on it. That's ridiculous. Of all the things that he could have chosen, he chose the one in order to see to it that he didn't have to refer to me. This happens in science. This happens.

Then, oh, I practically always have had something to say about how people acknowledge something, to try to make it a little bit more pleasant, a little more gracious. But that's a troublesome matter.

Lage: I'm not sure what you mean by that--how they acknowledged people. The actual wording of the acknowledgment?

Leopold: The wording. Yes. In other words, you can say things in a nice way that really is complimentary to the person that you're acknowledging, and there's some can be very brusque and as if you were doing this under pressure. I'm trying to teach people how you do things in a nice way. How do you be a scientist, for goodness sake?

UC Graduate Seminar in Geomorphology

Lage: Are these matters that you take up--you mentioned that you taught ethics to graduate students.

Leopold: Yes. The class that I gave at home, these were the matters, the kind of thing we dealt with. This was certainly the best class I taught. Later on I took only nine students. It got to be as high as eighteen.

The purpose of the seminar--it was started by Kirk Bryan in 1925, and I computed one time how many seminars there had been. It was carried on by all of his students up until the time that mine stopped, up until 1987.

Lage: His students, wherever they went, continued to teach it?

Leopold: Yes. But I always followed what he did more closely than other teachers. The way it started was that when you went into Professor Bryan's office, he had stacks of literature. He could read three or four languages, and he had stacks of literature. He would keep on talking--he was one of the most garrulous men I've ever heard--and he would grab something off the list and hand it to you, and say, "You report on this at seminar." I was there, for goodness sakes, I was there two weeks and I had a pile of five things in four languages that I was supposed to report at the seminar. It took me a half a year to find out he really didn't mean that. He wanted you to read them, but he didn't want you to report in the seminar on every one of the papers.

But anyhow, in my seminar I did something similar. Each person was to report on something that he or she read. I would usually try to find out what the student was interested in, and then help him or her pick out something that would either be right smack down his line, or something completely different than anything he knew about, so that he both had to do things that were right in his immediate interest and things that were quite far afield.

There was an oral report and a written report. I would say to everybody, "Your oral report must be exactly twenty to twenty-two minutes, not shorter and not longer, because that's what you do in a scientific meeting. If you say 'ah' or 'oh' or 'okay' or 'you see,' you're going to be stopped and you're going to start over again. I will not have any such cliches." And then I give them ideas about how to do this properly.

Well, I've had people, before they came up, go into the kitchen and throw up in the sink, they were so scared. I said to them, "Look, it's lots easier to have eight of your peers hear you, however well or badly you do, than to do it in a scientific meeting of 150 people."

Lage: You held this at your home?

Leopold: Yes.

Lage: And what was the title of the class?

Leopold: Geomorphology. A seminar in geomorphology. They learned a hell of a lot, and I heard people say, "How in the world do all of your students speak so well? They're the best speakers we ever hear at a scientific meeting." I said, "They've been taught how to speak well. They've been very carefully monitored to see to it that they were learning. Some of them have fallen by the wayside, I know, but they sure got it once and for all."

Then they would turn in a written report on the same thing. On those sorts of things, any misspelled word was ten points off before you began. A lot of students didn't like that very much either. I've always said I taught more English than I taught science.

The things that I learned from Professor Bryan's seminar at Harvard was he did too much talking. He was a very garrulous man, as I said, and the student wasn't given very much chance to talk. But in my seminar, after listening to the student make a presentation, then I would talk about the history of this idea; who had done what in the past, which is what Bryan used to do, too; speak about the problem of presentation, of ethics, of responsibility for your data; this kind of thing. So they learned a lot in addition to their learning how to present a subject orally. They learned about a lot of other things too. It was a very successful class, I thought.

Lage: Was there a separate class that you gave in scientific ethics?

Leopold: No. It was in this class.

Lage: You immersed it in the seminar on geomorphology.

Leopold: Yes.

Bitter Experience at the Survey after Leaving Chief's Job:
Isolation and Vindictiveness

Lage: I see. Okay. We don't have kind of a bridge of how you left the survey and came to the university.

Leopold: Very briefly, I'd gotten simply tired of being all alone, that's all, and they were causing a lot of trouble with some of my close friends like Herb Skibitzke, with whom I flew, who suffered the most degrading kind of things, all because he was a friend of mine.

Lage: Gosh, this sounds like a real terrible situation.

Leopold: Oh, God, it was absolutely awful.

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Leopold: Herb Skibitzke was in charge of a very small office in the Phoenix area, on groundwater. Being a very experienced pilot, as I've told you, he just thought that people who were doing the kind of work that we all do, they had to learn to fly. So he had a group around him that were all flyers--pilots--and as I told you, he had a wonderful photographic laboratory that he had made. He had this young woman who was about the level of a mid-career stenographer in the government service, and she turned out to be one of the best women pilots in the country. She was the first woman that ever flew an army jet all by herself. Amazing business.

When I first found out about what Herb was doing, I went to visit his office and looked it over. I came back to Washington, and I said, "I want to give a cash award to Skibitzke for what he's doing in this wonderful laboratory that he runs."

Lage: This is while you were chief.

Leopold: Yes. I got a reply from him saying, "I can't accept it because the only way I can do it is if you divide it equally among all my staff, including my secretaries." And then since I recognized he was a genius, I said, "Herb, forget about organization. You want something, you phone me." So he'd pick up the telephone, and of course the intermediate people got very angry at this. I said, "Look, Herb's different than the rest of us. Herb is a genius. He just doesn't operate very well in this kind of a hierarchical thing. This is the way we're going to do it."

Herb came to me and said this young woman that was one of his pilots, "I would like to see her promoted to"--what? GS-11, or anyhow, something that was like a high-paid secretary. Not even as high. No, about a GS-9. About as high as a first-year graduate would enter the government service in a scientific position. I said, "Fine." This was stopped by the intermediate levels in Denver. I spoke to these guys, and I said, "Look, I don't give a damn what you say. That woman deserves to be promoted. I'm going to promote her, regardless of what you people say. I promote her."

Well, apparently, the day I left the chief's job, I am told by people who were there, that there was a big cocktail party given to celebrate my leaving. When they all lifted up their glasses, they said, "We're going to get Skibitzke."

Lage: It's so petty.

Leopold: And they did.

Lage: So they got him--

Leopold: Oh, they got him, I'll tell you.

Lage: Now, how would they operate on him?

Leopold: Because of things of this kind. They accused Mary Lou, the one that I promoted. Herb had an airplane that the government had bought. He borrowed it from the army, and after we flew it for a long time--after all, these were all surplus airplanes--the army sold it, and Mary Lou bought it. She paid by check for the whole airplane, and then she went and she had it repainted and the engine fixed up. She was accused of stealing government property. Well, she went back to her checkbook, and she produced the check. She said, "What are you talking about? Here's the check." So they dropped that.

Then they accused her of having the thing painted at government expense, on and on and on. And then what happened was that because of all of the stuff we had--we had four airplanes and two helicopters and river boats and all kinds of equipment, and it was all on Herb's personal checklist, you see. For example, if you have a calculator, that's on your checklist, and when you leave the service you have to give it back. You have to account for it.

They were trying to get him to resign, or trying to fire him. They couldn't fire him because he was a civil service man, so they were trying to get him out. Herb said to me, "Luna, I can't get out. On my personal list I've got a million dollars worth of equipment. I'm going to make them sign for every gosh darn bit of it." I said, "You're darn right you do. Don't you do anything until they sign the whole thing over." So finally--it took them about three years--they finally agreed to, one by one, they took over the airplanes, they took over all the things.

The last thing they did was--. Of all these trips that Herb and I had taken, we had piles and piles of photographs and negatives from all of our river trips and all our trips to Alaska. One day when he was out on a trip for a couple of days, they took all this stuff and burned it.

Lage: Oh, what a loss.

Leopold: Those photographs I showed you are the only thing that was left. The negatives are gone. They destroyed them in order just to spite me and Herb. That's the kind of stuff that went on.

Now, this goes on in a government agency. The point is that these things were known to the supervisors. They would do nothing about it. They were part and parcel to what was going on. Anyhow, I'll tell you, it was a very bitter experience.

Lage: What about other people you'd worked with, like Raymond Nace?

Leopold: Ray was a wonderful man, excellent administrator. Ray retired. He died very shortly after that. He had emphysema; he was not a well man.

Herb finally got out after working for, as I say, several years to get them to take this equipment on his list. When he was running his laboratory after I left, administrators would come out to inspect him and they were saying this and that about him, Herb said, "Look, if you want this organization to run, we're doing things that are very valuable. But if you don't want to spend the money, just say so and we will shut down."

For example, they were making infrared pictures of the Everglades that were needed by a lot of people in the survey. One of the girls in his office--or two girls, as a matter of fact--they were flying helicopters, as I told you, in the dark, from Fort Barrow out into the Arctic Ocean to supply people on an ice floe that nobody could see because it's all dark in the middle of winter. They were using a hand calculator that Herb had fixed up. The radio was getting a fix from Australia and some other place in Brazil and Herb's calculator could tell them when they were a couple of yards of where they were. They were flying across the Arctic in the dark with nothing but this hand-held calculator and Herb's program.

So anyhow, Herb would say to them, "Look, if you want this operation to run, and we're doing things that are worthwhile, it costs you this much. If you don't want it to run, I'll fold it. But tell me. Don't just cuss me out and say that I'm spending too much money. I'm telling you this is what we do and this is how much it costs. If you want to close it up, close it up. I'm not arguing to keep it open, but if you're going to keep it open, this is what it will cost you to do it."

They got mad, for example, because Mary Lou flew an airplane--she was the girl I was talking about, Mary Lou Brown--flew an airplane to an air base in North Carolina, I think. She and Ruby, the two girls, got angry at being treated as if they didn't amount to anything. They had made for themselves a little uniform with the Geological Survey patch on it, you see. By God, they'd step out of an airplane, particularly if it's an army airplane, you see, and they'd walk across the field, and boy,

there's somebody. They've got a uniform. [laughter] She made arrangements with the army to sell gasoline for the airplanes at something like one-tenth of the cost it would cost everybody else. Instead of being glad of it, they were angry about it. She didn't go through channels, you see, to-- . It was a bitter experience for me, really bitter.

Lage: That was about five years?

Leopold: I resigned as chief in 1966. Yes, about that long, because I resigned from the survey completely in 1972, so that is about six years. Herb got out about the same time. It took about that long. Because things were happening to Herb Skibitzke and my own experience, I simply was fed up with it. And besides, I wanted to do conservation work, and I knew that to engage in lawsuits and that sort of thing and to help conservation organizations, I could never get approval. So I resigned.

To UC Berkeley in the Departments of Geology and Landscape Architecture, 1972

Lage: And then did you come straight to the university?

Leopold: No. I had an offer. I was making arrangements to go to Boulder, Colorado, to join the geology department there. Then my wife and I were not getting along at all, and she heard that Boulder had a lot of wind, and so she made me cancel that, and that's all right with me. But then I came here to give a lecture, and [Robert] Bob Twiss and Don Appleyard of Landscape Architecture called me and said, "You've written papers on landscape aesthetics which everybody's quoting, and I wonder if you'd join our department here." I said, "That's awful kind of you, but I'm not a landscape architect. I couldn't possibly join your department and claim to be a landscape architect. But if you want to make an arrangement with geology, I'd be glad to consider it."

Well, immediately they made an arrangement here, and through Clyde Wahrhaftig and Garniss Curtis they welcomed me here. So that's how I happened to come here.

Herb Skibitske and His Crew: Brilliant Iconoclast. Disturbing to the Survey Hierarchy

- Lage: Is there more you want to say about the survey?
- Leopold: As I say, it was very disappointing that all the things that we'd accomplished--many of those things were reversed, and the thing that hurt me the most was the way they treated my friend. That just made me so sick that I said I'd had enough.
- Lage: Were there other friends that they bore down on also? Or colleagues?
- Leopold: No. It all originated with the fact that I was treating this man of great capability in a manner which didn't suit the hierarchy.
- Lage: What was his position, actually? What job did he hold?
- Leopold: He was a mathematician on my research staff, and he ran a small unit. But because we had so much fun, you see--we all flew airplanes, the two girls--
- Lage: Everyone was envious, most likely.
- Leopold: Probably. Oh, yes. They were indeed, yes. We ran river trips.
- Lage: Flew to Alaska.
- Leopold: We went to Alaska a lot. They were very competent people. For example, after many years, a couple of years ago, Ruby Shelton, one of the two girls, finally won the Women's Powder Puff Derby, flying across the United States. She's the only woman in the world who ever was granted an instructor's license to fly a helicopter under instruments. The only woman who ever did that.
- Lage: Was she also promoted from stenographer or something like that, or did she come up through a--
- Leopold: It was worse than that. She had had an even lower position than Mary Lou. Oh, I know what happened. Ruby was the only woman in the Southwest who had such a reputation with the Federal Aviation Administration--FAA--that she was not only instructor, but she was also a reviewer for FAA. But in order to try to get rid of her, to make Herb and me mad, they sent some nincompoop from someplace out to test her.
- Lage: Test her flying skills?

Leopold: Test her flying. Well, Herb had equipped a Mohawk. A Mohawk is an airplane, a propeller airplane, that was used primarily for remote sensing. Herb had himself invented--or let's say did the programming, all the electronics, to produce in his airplane the most advanced side-looking radar of anybody in the world. He had taken little bits and pieces that the army was discarding from their very highly secret stuff and he put it all together, and he had one that was better than the army's. And he had it in this Mohawk.

Well, this nincompoop came out and he was ostensibly going to give Ruby a test. They were up in the air, Ruby was flying, and I don't know what he did, but he pulled a lever someplace and it opened the hatch under flying conditions, and both of them were nearly thrown out of the airplane. She suffered a very severe back problem, and they never would pay her for it.

Anyhow, she said that it was absolutely incredible this guy did something so unsafe. To pull a lever in an airplane that he had never flown himself, that he didn't know what would happen, in an airplane under her control--. And he did something that no flyer with any sense would ever think of doing, and he was supposed to be testing her. Then, as I say, they never would acknowledge her injury as in line of duty.

Both of the girls quit. When Herb finally quit, they both quit. Ruby never had been with us long enough to get a full retirement. She's just been struggling along. Mary Lou had been with us a long time and she also had some money of her own so she was quite well off. But I always felt so damn sorry for Ruby because she--. You see, if I had been chief, it would have been entirely different. And they would never acknowledge that this thing had been done to her and that it was an accident in line of duty, so she never got any compensation for it. That's the kind of thing that happens. When I start thinking about it, it makes my blood boil.

Lage: You probably put it out of your mind for quite a while. Bringing it back like this must be painful.

What did Herb go on to?

Leopold: Herb is way and away the most talented consultant in advanced groundwater hydrology in the world. Last time I saw him, in his office in Tempe, Arizona, he said he had just finished a job. He writes these computer programs to describe a groundwater system. He said that the last job he did, the program that he just wrote was six thousand lines, if you can imagine keeping in your head the sequence of six thousand items.

Lage: He sounds like a fascinating person.

Leopold: He's fascinating. The University of Arizona has really been pretty good to us. They finally gave Tom Maddock an honorary degree, and they finally gave Herb an honorary degree. Herb Skibitzke didn't have a degree at all. I was trying to get him up into a higher grade, and they wouldn't promote him because he didn't have any degree. So I said to him, "Well, Herb, you're going to have to get a degree." And I've forgotten what we went through, but I told him to study certain things. Somehow or other, he worked very hard and got a degree.

So then when he got into this groundwater consulting work, he found that the people that really were getting most of the work were engineers, so he decided he was going to be an engineer. So he sat down and studied for a couple of months and he took the written examination for civil engineer and passed it, just without going to school. He passed it. Hell, I couldn't any more pass that test. I couldn't possibly pass it.

Lage: Amazing. Do you see him often?

Leopold: I try to. I certainly phone him rather often. He's an awful good friend. When I was in the hospital for my hip operation, I didn't even know anybody knew about it. Herb flew from Phoenix to come and say hello to me for a day. Just a couple of hours, just to be thoughtful.

Leopold: I can remember on the great Alaskan river expedition, Herb was flying over the boat a couple of thousand feet high, and he said to me, "Say, Luna, there's a great big moose over there on the right, just about a quarter of a mile ahead of you." I called him back and I said, "That's swell. We'll go look at the moose, but I don't want anybody shooting anything around here. We're not going to shoot any moose or any bear or anything."

So we all got out of the boat and we went sneaking through the timber until finally we came to a great meadow. I could see Herb flying over me. We walked across that darn place, and we didn't know what that damn moose was going to do. He was the biggest moose I ever saw in my life. I had my rifle at my shoulder like this, you know. The moose just looked at us and he wandered off. He didn't care about anything. [laughter] We used to have a lot of fun with the airplane flying over, and he could tell what everybody was going to see.

XI THOUGHTS ON A HALF CENTURY IN HYDROLOGY

[Interview 8: May 9, 1991]##

Overview of Contributions to Geological Survey and Field of Hydrology

Lage: We were going to talk today about some of your most important papers, those we haven't yet discussed.

Leopold: Before talking about individual papers, I think it would be worthwhile to say a few words. You asked me in your last outline request about what I thought were the important things that happened. I would say the most important thing was exactly what we just spoke about, and that is the changes in the Geological Survey, the transformation of this important organization from one that was very restrictive in its viewpoint to one which now has expanded its research outfit into a much larger unit than even I had imagined. That was an important and lasting contribution.

The second, and we've mentioned this before, is the training of individuals, which has turned out to be important to science and important to universities, because many of them have gone to universities.

Lage: To teach, you mean?

Leopold: Yes. And then again we've mentioned, but we'd just as well summarize here, the idea that I had of the long-term recording stations, which we called the benchmark gauging stations and the benchmark basins. In combination with that, the vigil network, which is again the accumulation of long-term records.

Now, to my great surprise and disappointment, the university at Berkeley has dropped the rain gauge which was on this building since 1886. I wrote a long letter to Chancellor Tien and said,

"This is the kind of thing that represents the heritage of Berkeley, and it's a shame after all these hundred years."

It has been 104 years that the station was run without a break. And now all of a sudden the university said, "It's not important." And then to see that the people upstairs in the Department of Geography don't think it's important, and they are the ones who are responsible for it. And we who are on the outside must turn and try to persuade the administration. This is exactly the opposite of what we tried to do in the Geological Survey, and somehow or another, the idea of having no value attached to long records of something of real interest--. It's the longest rainfall station in California. It's a shame.

Lage: I couldn't imagine that it's terribly expensive to maintain it.

Leopold: No, they've been spending \$6,000, and I asked the chancellor if he would put up \$5,000. He said no. On the other hand, the chancellor had somebody else write to me. He didn't call me himself, which I'm sorry to say. I asked for an appointment, and failed. Anyhow, that's the kind of thing that is too bad, because all the things that I have been talking about as important in the Geological Survey now are demonstrated to be unimportant to this university.

Lage: That's very disturbing.

Changing Geomorphology to a Quantitative Science

Leopold: Now, turning to one other thing that is important and that relates to the problem of the papers that I wrote, I don't think there's any question about the fact that my first series of papers in the Geological Survey changed the field of geomorphology from essentially what we called an arm-waving pastime to a quantitative science. It started the quantitative geomorphology that we know today along quite a different route.

Lage: Now, which papers were these?

Leopold: There were a series of papers. In the first place, you remember I told you that no Water Resources Division employee had ever published a paper in the professional paper series. After having looked up the purpose of the series, I went to the director and said, "That's silly because the series is open to anybody in the Geological Survey, and this is a paper written for geologists to explain something about water."

So we started, then, the idea of publishing scientific papers in either the professional paper series or the water supply paper series. So the first three papers I published in that series were the paper on the hydraulic geometry of stream channels in which I was showing how we could use quantitative data collected by the Water Resources Division to see new things about river channels. The second paper was a quantitative study of the characteristics and hydraulics of ephemeral streams in the Southwest--streams that flow only a few times a year when it rains. The third was a paper on stream channel patterns, which included the laboratory work I did when I was a visiting professor at Caltech.

Those three papers really got the thing started. In other words, this was really the first of the long series of quantitative reports on hydraulics and geomorphology that were published by the Geological Survey. So I would say that those three papers were important in several ways. They represented a variety of viewpoints.

This brings up a matter of importance. I recognized very quickly when I started to send people to school that one could expect that any person just getting a Ph.D. is going to want to spend a year or two extending the work that he did on the Ph.D. But the test of a person's scientific breadth is going to be what else he does. The people who stick strictly to the things that have been the subject of the Ph.D. or Master's thesis are too restricted. What one looks for, you are looking for people who begin to spread themselves out into various aspects of their science, and that, I think, is one of the hallmarks of what I was trying to show, that actually you could do many things.

Lage: Did these three papers grow out of your Ph.D. work, or were they totally--

Leopold: They were totally separate. The only thing that grew out of the Ph.D. work was my first paper in the water supply paper series on what I called the post-glacial chronology for some alluvial valleys in Wyoming, which was a study of terraces. And again, this kind of broke some new ground in the Geological Survey. Here was a paper that dealt with geology. It was published in the water supply paper series. I was trying to show that geologists should be writing for hydrologic engineers. My first paper in the professional paper series, which was read primarily by geologists, was a paper on water. I was trying to show that there has to be greater cross-connection between geomorphology where it's an aspect of geology, and hydraulics. And of course,

everything that's happened in geomorphology since has been a melding of those two subsiences, geomorphology and hydraulics.

Lage: Is it correct that there really was not a science of hydrology until you brought it together?

Leopold: No, there was a science of hydrology, but hydrology was not considered to be related to geomorphology. In other words, hydrology was the science of building reservoirs and dams.

Lage: Which is more like hydraulic engineering.

Leopold: It's hydraulic engineering. The great textbook that was written in the late twenties by Daniel W. Mead, called Hydrology, was everything that we do in hydrology today but it was directed specifically to the engineer who was developing water supplies or flood control works. But it was not related to geomorphology. It was not related to the processes on the earth's surface. It was related to engineering matters.

Similarly, preceding my book on geomorphology was the great addition to hydrology written by Ray Linsley, who was with the Weather Bureau and then at Stanford. That was again simply a furtherance of the Mead idea of hydraulic engineering.

Lage: How about hydrology in Europe? Was it slanted towards the engineering aspect?

Leopold: The Europeans were very skilled in their work on rivers, and that is hydraulics, but again, toward hydraulic engineering. But they were not in general relating earth surface processes to hydraulics. There were some very important things that came out of the Europeans in the latter part of the nineteenth and early part of the twentieth century. The great river study in France, and some extremely important work going on both in Switzerland and Germany. But again, they were not related to earth surface processes. Really, what geomorphology is about is the earth's surface. So that was really an American direction, which was, of course, immediately picked up by the Europeans, and they've done extremely well with it.

Lage: One of the things Dave Dawdy [see appendix for interview with Dave Dawdy] mentioned was your continuing influence by influencing the development of university programs. Did this idea of hydrology sort of become institutionalized through developing university programs as well as through the Geologic Survey?

Leopold: Yes. The university programs for the most part developed in the direction of hydrology, not geomorphology. Geomorphology really picked up later. Geomorphology really didn't begin until some of the people that I had hired who worked for the Geological Survey had left the survey and gone back to the university.

Lage: Who would be the most important people there?

Leopold: M. Gordon Wolman at Johns Hopkins, one of the men that was associated closely with me and worked with me for some time and then went to go to Hopkins and has been chairman of the department there for more than twenty years now.

If you look at the first training school that we set up at Arizona, it was primarily groundwater and surface water hydrology, with a great emphasis on groundwater hydrology. The Geological Survey had always been the premier organization dealing with groundwater. That resulted from several individuals over quite a long time. Because I'm not a groundwater hydrologist, I can't say very much about that except that it appears to me that the real advances of the last two decades have been made by university people, not by Geological Survey people, although they're always building on the great work of Meinser and Theis, both Geological Survey people.

But going back to individual papers, another thing that was moderately influential at the time were the general essays I was writing on the subject of water in general. Unfortunately, that has not been followed up very much. The survey has not recently been a spokesman in the general field of water and water development. That has been really taken over, if you like, by---. Well, the people who have emerged in the last forty years have been people like Gilbert White at Chicago and later at Boulder, and then especially Europeans.

Lage: Which papers would these have been that you wrote in the general field of water and water development?

Leopold: It started out with the series called "Water and the Conservation Movement." There was a series of essays about that.

Lage: Right.

Leopold: The work that my associate, Ray Nace, did on the Hydrologic Decade was very influential, especially in Europe and in other countries. But that was not as much my work as Nace's work, but he being my associate chief, he simply took that on as one of the main contributions he wanted to make, and everything that I could do to support him was what was done. But it was primarily his

work and was very influential in furthering hydrology, especially in other countries. It made less difference in the United States.

Lage: What was the nature of the Hydrologic Decade?

Leopold: The Hydrologic Decade was a ten-year emphasis by UNESCO on the question of water in the world. There were a great many things that were done under that decade. There was, for example, surveys made partly under the auspices of the Geological Survey of the flows of the great rivers of the world, the sediment content of the great rivers of the world, the chemistry of the rivers of the world. Those were important contributions to the knowledge of water in the earth. In that connection and as part of that decade work, Ray Nace himself made a tabulation of, collecting from all over the world, the flows of all the rivers of the world, which has not been changed very much from what he did then. So that was an important contribution made by the survey.

Shortly before I became chief hydraulic engineer, I was approached by the Conservation Foundation of New York. The Conservation Foundation was hiring authors to prepare a series of books on various parts of hydrology, land management, and water in general. They'd just finished supporting a scientist who was working basically on infiltration and run-off. They came to me and asked whether I would do a book on flood control, because at that time there was a lot of controversy in the Congress and around the United States on the matter of the work of the Soil Conservation Service and the work of the Corps of Engineers.

After some discussion I said I would do it, but I wanted a co-author, and this will bring up another thing I'll mention. So I enlisted my friend Thomas Maddock, Jr., and we did this book on the flood control controversy. [The Flood Control Controversy: Big Dams, Little Dams, and Land Management (New York: Ronald Press Company, 1954).]

Lage: Did you take a leave to do that?

Leopold: Yes, I took a leave from the survey to do it, yes.

Lage: Was there a reason for taking the leave?

Leopold: A government officer can't accept money from anybody, and in order to--and I didn't get any more money than I would have gotten from the survey, but I had to separate myself from the survey. I took leave without pay and was paid for a year by the Conservation Foundation.

Well, that book certainly held a lot of truth. It had some impact. I remember the chief of the Corps of Engineers saying to me one time, "I don't mind you being so critical of the Corps of Engineers as long as you're equally critical of the Department of Agriculture." [laughter]

Lage: So there was a lot of interagency rivalry going on there.

Leopold: Oh, yes, there was indeed. Actually, you could read that book now--what, 1954--fifty years later, and it's just as true as it was then. As a matter of fact, everything we've said is even worse, perhaps, than it was at that time.

Lage: Were the recommendations you made controversial at the time? You did come forth with some public policy recommendations, one being that those who benefit from flood control should pay the cost.

Leopold: And that has gotten nowhere. Or only until recently, until in the budget crunch of the last five years. Then the Corps of Engineers started to change their tack, and it's much more difficult to get straight-out government grants, which used to be in the order of 92 percent. Ninety-two percent of the money was carried by the taxpayer, and the recipients were paying only upkeep, operation maintenance, and the cost of the right of way. That, of course, got much more stringent on that, but that took fifty years before it changed.

We recommended flood insurance. Well, that took many years before that got going. Again, I'm far enough away from that program I really can't say how successful the flood insurance program is. I told you that Langbein and I had tried to devise a hydrologic scheme to divide the cost on the basis of risk, and that was not accepted.

One thing that we did show--and after that, nobody ever argued about it again--was that the small dams upstream can't take the place of the big dams built by the Corps, that they do different things. That was quite unclear at the time, so in that respect, that was an education that the American public needed. Now, how much of the public, I don't know, but for the people that were interested in that subject, that problem was laid to rest by that book.

Lage: Was that something that the Department of Agriculture and the Corps of Engineers had been battling about?

Leopold: Yes. You see, there were a lot of people who said, "We don't need any big dams at all. We're building hundreds of dams in the

upstream parts of the watershed, and if you build enough small dams upstream you will solve the flood problem." But the book also brought out some very disheartening things going on in public policy that really weren't corrected for a very long time afterwards. The problem was that a lot of money was spent on a few individual farms, and a few people got the benefit from a lot of money.

Lage: On flood control for these few farms?

Leopold: Yes.

Lage: How did you go about your research on this? Did you look at a lot of individual projects?

Leopold: No, no. When I took the job on, I had one thing in mind: I was going to make a study of what had been found out by the dozens of experiment stations that had been running all over the United States for many, many years. No one had ever attempted to find out what the experiment station data really showed. So I made a survey of all of the experiment stations and their data from all over the United States, and came to some very disheartening conclusions. One was that much of the data went to waste. I forget what the numbers were, but not more than 10 percent of all the data had ever been published. Half of the data had never been looked at or analyzed, and a tremendous lot of money was going into data collection for very good purposes, but no one was doing anything with the data.

Lage: These are agricultural experiment stations?

Leopold: Yes.

Well, the second thing I wanted to do was to actually make hydrologic and hydraulic computations to show what would happen from a series of dams, and that has been reproduced many times over. That turned out to be a very successful study showing how a series of small dams operating under different conditions of rainfall had different flood effects downstream, all of them dying out rather quickly, so that people immediately downstream from the dam got a lot of protection. A little farther downstream, they got no protection whatsoever.

Then there was the problem of general education, which I attempted to do something about. I guess I told you the story that I wanted somebody to write a primer on water. Nobody did it, so I did it, and it turned out to be extremely successful because it was a way of educating people by writing a simple story about water.

Lage: Was that used in the educational system? Do you know what audience it ended up with?

Leopold: It did, but I couldn't tell you in any detail. It sold more copies than anything the Geological Survey had ever published. Not sold, it was distributed. It was free.

Lage: Oh, I see.

Leopold: And then later I revised it somewhat and turned it into a small book after I left the Geological Survey. [Water: A Primer (Freeman series on geology, 1974)]

Entropy and Landscape Evolution

Lage: What about "The Concept of Entropy in Landscape Evolution"? That was 1962 [USGS Professional Paper Series].

Leopold: That undoubtedly was the most important idea I ever had. It's still so difficult to understand that it's not either very much read or much understood. But in the long run, science is going to have to come back to something like this. The idea is that in the hydraulic system of a river, everything, of course, is governed by physical laws. The physical laws describe interaction between various parameters such as depth and velocity and roughness and sediment load, but it turns out that, as in many other cases in the world, the physical relationships do not handle all the variables.

Therefore, in many things in life and many things in the physical world, it turns out that probability has a great effect. The example we give to make it clear to people about entropy is that entropy is simply a statement of organization. We said in that paper that if you distribute your material on your desk, it gets more scattered and therefore is increasing in entropy because there's no organization.

Lage: So entropy is lack of organization.

Leopold: Right, lack of organization. And then when you take the time to organize your letters and put them in files, you're having to expend energy to do that. But what you get through this is an organization, a segregation, the differentiation. But in order to get differentiation, you had to put some energy into it. You had to take time to do this. So that the more organization you

have in your files, the more work you had to put into it. If you don't do that, the thing goes into a more and more disorganized state.

Now, because of that general principle, it turns out that in the river system, there's not just one answer. If you had all the equations that are needed, then you would say, "If you're given these set of circumstances, the river must do thus and so." And that's not so. If you're given certain circumstances, the river has several choices, and that choice can be dictated by chance, and indeed is dictated by chance. Now, that is a very important idea because it means that the river has a lot of internal flexibility, all governed by physical laws, but the laws don't make it clear that the river has to do certain things. It can get wider, it can get narrower, it can get faster or slower, it can carry more load or less load.

Lage: And this is all chance, not the physical characteristics--

Leopold: The physical characteristic does not dictate that it has to do only one thing.

Lage: We talked about random walk. Is that what this--

Leopold: That's right. That's exactly it. Yes.

Lage: This is such an interesting concept.

Leopold: Yes, that's a part of it. It turns out that the organization of the river in that work is random. It's not dictated, it's random.

Well, this led to a lot of things that have been picked up. The easy parts have been picked up and have become very useful. The idea that we could develop a river network by throwing dice, or turning cards. Now this has been done by computers, and people have done this all over the world. By the introduction of the most probable case.

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Leopold: The most probable case can be described in general terms in this manner. When you have a scatter diagram of X against Y--for example, what is the relationship between a person's height and his age as he grows, and you get an increase in height as the person increases in age. Given the scattered data that we have, let's draw a smooth line through it. What is the line of best fit?

The line of best fit is called the line of least variance. Least variance is a very specific, statistical statement. It says that if you take the deviations of each individual point from the main line, and square those deviations, the sums of the squares of the deviations is minimum, and that is the best line of best fit. So the line of best fit is the minimum variance line, the line that minimizes the variance between the scattered parts and the smooth line that you draw.

Now, it turns out that that's exactly what happens in rivers, that what you have is that the most probable case is the case where the variance among the variables is minimum, and that's what we had to prove. And it turned out that the variance indeed was the exponents of these parameters that I proposed in the hydraulic geometry equations, that the exponents themselves were the variance. The measured variance.

Lage: Does this work that you did relate to broader ideas in science? You hear a lot about theories of chaos now.

Leopold: This is all just an example. This is very closely related to chaos.

Lage: But was chaos that big a concept at the time?

Leopold: No, chaos followed this. But there are a lot of--

Lage: Were a lot of different fields coming to the same conclusions at the same time?

Leopold: Yes. They used different words, but in many cases they are comparable. Now this famous man, Benoit B. Mandelbrot, who developed the idea of these beautiful fractile patterns, had another idea that I worked on for many years and I never was able to do anything with, all related to the same thing, and that, again, a distribution. He was discussing a general problem in distribution of numbers that seemed to me very closely to fit certain river data that I had.

A lot of these things are all operating together. Fractiles and chaos and entropy are all--. You're using different words, but you're really coming to something very similar. Chaos simply says entropy. Disorganization is what it's about. Therefore, there are certain things that happen in disorganized states that actually have an appearance of being organized, but I don't know enough about chaos to speak about that.

But you are correct in saying that there's a very close analogy between a lot of things that are going on in the physical

sciences and in the mathematical sciences, and they all relate to this general idea of the interrelationship between organization and disorganization, which means organization and chaos, probability and improbability, and what happens in nature, which is not entirely deterministic. Nature is governed by laws of physics and chemistry but the outcome is not deterministically fixed.

Lage: Do you think these ideas come up in the different areas of science at a similar time because of cross-fertilization?

Leopold: Yes and no. I'll give you an example. I've always felt that the way to get ahead in my field is to search other fields and see what ideas you can pick up, because you're going to find something that you can use if you knew something enough about somebody else's field. And entropy was one of them. Entropy is something well studied in physics. No one had ever applied it to hydraulics. Now they talk a lot about it.

Lage: But why did it appeal to you at that time to apply it to hydraulics?

Leopold: Because that was a general point of view that I had, is that I'm always going to find something that I can use from somebody else's science that will apply to my science, if I know how to look. You just have to keep looking. As I told you, the way I arrived at this was out in the field, where I made an observation in meanders that no one had ever seen before, and I said, "The river's trying to do something. What is the word to describe the river trying to do something?" Well, it's trying to organize, isn't it? It's trying to balance something. Well, that balance is entropy. As I explained to you, the river is attempting to balance the tendency for least work against the tendency for the most uniform work. That's a balance, and that's a statement of entropy.

Lage: You mentioned that somebody suggested this idea of the random walk in a discussion with you.

Leopold: Yes, I was talking to Harold Thomas from Harvard.

Lage: What field was he in?

Leopold: He was a former professor of mine.

Lage: He was in geology?

Leopold: No, he's in the field of hydraulics. But he happened to be using that idea of random walk to study the movement of water through

sand, and I thought, now that you mention that, I can use that idea. In other words, it came from another science, but you've got to keep always looking for things that you never thought of before that might be applied to your science even if you don't know exactly how to do it yet.

Lage: Somehow it seems that some of these ideas not only fit the science but they fit the philosophical outlook of the times. I wonder if that has any relation to why people pick up certain scientific ideas to apply.

Leopold: I'm not sure exactly what you mean.

Lage: Well, I'm not sure either.

Leopold: But you can say this, that there are people who are trying to influence the thought of the times, and these are how these influences operate. You pick up something that no one ever thought of before, and then all of a sudden it spreads out, and then it becomes the idea of the times.

Lage: Right. That's very exciting.

Hydrology in Urban Areas: Study of the Brandywine Basin

Lage: I made a note here to discuss "Hydrology for Urban Land Planning" (1968), because we really haven't talked about your work relating to the urban end of things.

Leopold: Yes. Well, that came up--.. And then again, this is jumping into fields that don't belong to you when opportunity arises. Some people from the University of Pennsylvania came to me in the office one time and said, "We've been going all over government trying to get somebody to help us in the problem of land planning that we are trying to get started near Philadelphia." I said, "Wonderful. Let's do it together. I'll furnish the hydraulics."

So anyhow, I immediately became involved in this large planning effort for the Brandywine basin near Philadelphia. We worked very hard at it for several years, published a big report. The Ford Foundation was going to put up the money to buy easements in order to protect the land from overdevelopment. As usual, a group of people thought that this was imposing on their individuality, and they started a newspaper to knock us down. When it came to a vote, we were defeated. Now, twenty-five years later, they're trying to do exactly what we were doing and, of

course, by now urbanization has gone on at such a rate that the thing is much more complicated, and furthermore, they don't have money behind it like we did. It was very, very frustrating.

Lage: So which ones objected to the purchase? Was this sort of a conservancy idea?

Leopold: Yes, it was a kind of conservancy idea. What we were trying to do was to say, "We're going to have some rules about how development is going to proceed." These are rules, many of which I had to devise and my friends had to devise; we didn't know exactly what the rules ought to be. How close to a stream channel would you be allowed to build? I said, "Three hundred feet." This had come back to haunt me again and again. People come back years later: "How did you decide it was three hundred feet?"

Lage: How did you?

Leopold: I said the literature showed that in soil that has a certain amount of clay in it, pathogens moving with the water are absorbed by clay within a distance of between a hundred and 150 feet. I said, "Let's just be a little safer than that and let's make sure that any pathogens that come from housing do not get to the stream channel but would be absorbed." So I said just arbitrarily, "I'm going to choose three hundred feet." It turns out it was a very good rule.

Well, then we had rules such as on steep slopes, you could not clear the timber or build buildings on slopes that had certain characteristics, including their steepness. We were concerned with where to put roads. In that particular type of topography, we felt it would be far better to put the roads up on top of the hilltops, not on the hillsides, and not down near the channel. They were perfectly simple things of this kind.

In order to accomplish this, we were going to have the local people--. I forget exactly how the thing worked. We were going to have the local people sell some of their rights in order to be members of this plan, but the rights they would sell were for their own protection, but they would get money for them.

Lage: But it would protect their property?

Leopold: Yes, and it would protect the region or their community. We figured at that time that a scenic easement, for example, should not cost more than 50 percent of the land value. I heard last week in Philadelphia when I was there that in the same area, the local people are asking 95 percent. So if your land is worth

\$10,000 an acre, in order to sell you a scenic easement, for God's sake, they want to charge 95 percent of that. Well, that's ridiculous. In other words, they still have the land. They're not giving up the land.

Lage: Right. And actually, their land will probably go up in value as a result of the scenic easement.

Leopold: Well, anyhow. So that involved, then, developing some hydrologic procedures for land planners, so I wrote a paper called "A Handbook on the Hydrology of Land Planning."

Lage: Did you work with somebody on that?

Leopold: No. That was my own paper. That, of course, has been now expanded by quite a few hydrologists, too. The latest one, I just received a reprint of the paper that I just published in Germany on the effect of urbanization on the campus here in Berkeley. I spent ten years studying that and it's just now been published.

Evaluating Non-Economic Values

Leopold: Well, then, the other thing that I did that was really quite different was the problem of aesthetics. That really broke a lot of new ground.

Lage: Let's talk about that.

Leopold: I was very disconcerted that in the planning that I saw going on in government agencies, there was a tendency to always want to put a monetary value on everything. A goose was worth \$4 and a mallard was worth \$1, and this sort of thing. Well, my father became famous in trying to get away from the same thing, trying to say that it has an intrinsic value of its own. So I wanted to do something to develop a procedure by which we could make relative evaluations from the purely human point of view without having to put dollar signs on them.

Unfortunately, in many instances, the reaction of the readers came out to be, "Well, you've gone that far. Why don't you put dollars on them?" I said, "That's exactly what I'm trying not to do. I'm trying to rank them so that you'll say, 'This is better than that' for very objective reasons." So I wrote a paper on how this might be done.

Lage: Was that your paper on Hells Canyon?

Leopold: No, that came later. This paper was the first paper on attempting to do this in a quantitative manner.

After that had been published, some very prominent planners and people in Resources for the Future came to me and said, "We need the kind of help that you've been working on, on this Hells Canyon problem, because the Federal Power Commission is going to have a decision as to whether they're going to allow a dam to be built." They asked if I would consider the matter. Well, since I already had started out on this thing, I said, "Here's an example of how we can put it into practical use. Let us make a study and make some kind of a relative comparison without money attached to it of what is valuable in the way of wild country." Well, that ended up in that Hells Canyon paper. But that hasn't been much followed up. I guess I told you that the general plan was used explicitly by the Park Service of Canada, when they were laying out national parks in Canada.

Lage: Another one was "A Procedure for Evaluating Environmental Impact," [Leopold, Frank, Clarke, (USGS, 1971)]

Leopold: That again, that followed the same kind of thing, where after I wrote the paper on the Florida Everglades, which was the first environmental impact statement ever written, then there became a great hue and cry about environmental impact statements, and the question is, can we give people any advice as to how an environmental impact statement might best be done? The feeling that several of us had was that the people writing environmental impact statements were not really considering the interaction of man's activities.

So we prepared a great checklist which was similar to the kind of thing I'd used in the previous papers. A checklist where you say, "Here are some items, and I'm going to evaluate them. I'm going to give them some numbers, some relative ranks." So we tried to show that if you made up a very large matrix of causes and effects, that there were certain combinations that you could say, "These combinations apply to this particular job. This is what's happening here." We gave them some general hints as to how they might evaluate these kinds of combination.

In one example I recall, we pointed out that these ranks have a lot to do with just how you see the issue. One example we used was the question of mining down in the California coastal mountains where the condors were. The example I used there was, there isn't anything more important than the condors. In other words, this is a species nearly extinct. All these other issues

become subsidiary. This is the kind of decision you have to make. If you're going to rank things, you have to state what your ranking is.

What I object to, and have done so in many papers over a long period of time, I object to people saying, "This is the most important consideration," when they don't tell you why they arrived at that. What I've tried to say in that paper that you speak of is, "I don't care what your answer is. Please tell us how you got there." In other words, "You think that this is the most important thing. All you have to do is say, 'This is what I think is most important, and I rank this number one.' At least then we know how you got there." But just to try to tell the public that this is the most important, that such-and-such is the most important thing, without explaining how you got there or why you think so, seems to me is not supportable.

Lage: Has that trend continued, or have we gone to putting dollar values on aesthetics?

Leopold: I'm afraid that we continue to put dollar values on--

Lage: There's a whole field of resource economics now that seems to be working at that.

Leopold: Yes. And there have been some very clever ways of trying to make estimates of dollar values, which is all to the good. But as soon as you do that, you're making the implicit statement that everything can be reduced down to dollars, and that, at this point, isn't true in the human condition. There are certain things that simply are not valued in terms of dollars.

Lage: So the resource economists are comparing these aesthetic values to production values or other resource values, and you're saying no, they--

Leopold: Yes. In other words, the criteria really should be different. There are certain things that are simply not of that order. They're not of the same nature, and in my opinion, therefore, you should have different ways of evaluating them.

Lage: But then how do you compare them with each other?

Leopold: Then you can say all right, now, given a set of values, then we can start talking about what's gained and what's lost in some relative terms. Have you read the paper by my father on the land ethic?

Lage: I have.

Leopold: All right. Now, that's the kind of thing we're talking about. Humans place value on some things just because of our innate feelings. In other words, you have to feel that there's value in the different parts of the biota. Whether or not you either understand it or whether you find it economically valuable in monetary terms, that's really just the thing we're talking about. We're talking about a set of values for humanity that simply are not going to be measured in economic terms. Once you admit that, then you are going to have to say, "All right, if that's so, then how do we go about doing it? How do we make evaluations?" And that's what I'm trying to do.

Lage: Are there other areas of your research that we should discuss that we haven't? Or another approach to this would be, when you get the transcript of these interviews, if you think something's missing, we can add it in.

Leopold: Yes.

Lage: I think we've gotten a pretty good overview, but we may have some gaps that we could fill in later.

Leopold: Right.

Family and Family Values

Lage: Now, let's see. I had wanted to ask you more about the personal side of life, like the building of your cabin on the New Fork, and family type of things. Your own experiences in passing on some of your feelings and values to your own children. Is this something we could talk about?

Leopold: I was married for many years to a girl that didn't like this business of field work. She really prevented my two children from going out in the field with me, so that--

Lage: It looked like you took Bruce along a lot, from the journals.

Leopold: At one stage, yes, but not as much I would like. My daughter, Madelyn, came along on her own very well. She has a very good sense of values now, but--

Lage: But she didn't go out in the field with you?

Leopold: No, she was prevented. My wife simply never let her go out in the field with me at all, which was a real shame.

Lage: And what about Bruce? Did he develop along the lines that you and your father--

Leopold: No, he didn't. Bruce is a real hedonist. He's very much concerned about his own joy and welfare. That's just a personal way of looking at it.

Lage: Like so many people are.

Leopold: Yes, so many people are.

Lage: It's a very hard thing to pass on, I think, and your father was extra successful at it, for whatever reason.

Leopold: For whatever reason. And of course, I don't think any of us really understand what these reasons were. It just turned out that way. I don't know. That's been discussed ad infinitum. I really can't make any general statements about that. One thing is that it requires a certain amount of humility to admire somebody without being jealous of them and not trying to necessarily either equal or outdo them, but to gain what you can as best you can. I think that the five of us really looked at our father that way. We knew that we were never going to be able to write as well as he did, yet we were going to try. We were admiring without being envious. I think that's a very important matter, but somehow that's got to be built into you. I don't think that's something that you get taught.

Lage: No, I don't think so either.

I saw a mention in your journal, about a trip that it seemed a lot of your brothers and sisters were on, in Desolation Canyon of the Green River?

Leopold: Yes.

Lage: How did that come about?

Leopold: I simply asked whether anybody wanted to take a trip. It was after I was no longer with the Geological Survey. One of my rivermen, Smuss Allen, still had a boat, so I hired him to use his boat to take us down the river. We had a good time.

Lage: Who all came?

Leopold: My brother Carl, Estella, my wife Barbara, Rett Nelson and Carrie Nelson, my wife's children, and one of my close friends from Canada, Denny St. Onge, a geologist. I don't know. I get many of the trips mixed up. It was a family trip.

Lage: The photo I saw had Starker and Estella and Carl. Was that a different trip?

Leopold: That was another trip. That was a hunting trip. It also was the Desolation Canyon.

Lage: In '65.

Leopold: That's right. And that was Frank Clarke from the Geological Survey. That was when I was still with the survey, and I had a lot of survey people. I wouldn't do very well in the present administration because if you look back at some things I did, this business of Mr. John Sununu [chief of staff to President Bush, criticized for using government travel for personal business], they would have--

Lage: [laughs] They would have gotten you, huh?

Leopold: They would have clobbered me, yes. They could have said, "You know, you're doing this for your personal use." As a matter of fact, one of the interesting things is that when I was with the survey, no river trip--and we loved the river trips, of course--no river trip was taken, but what I wrote a technical paper about it. So that there were real scientific things that came out of it, but I think you'd have a hard time selling that to the United States Congress if somebody started to object.

Lage: You're not supposed to have too much fun.

Leopold: You're not supposed to have too much fun. Exactly.

Building Cabin and House in Pinedale, Wyoming

Lage: Okay, let's see. What about your cabin on the New Fork? How long have you had that, or is it a house?

Leopold: I've got two. I've got a cabin and a house. First John Miller and I camped on Pole Creek for quite a few summers. We were young then and we had no tent; we just had a little piece of tarp for a lean-to. It was pretty rugged. Well, Reds Wolman and I camped on Pole Creek a couple of summers. By this time, it was

getting pretty difficult. It was great fun, but I wanted something a little bit more convenient.

It happened that I was visiting my sister, Estella, in Denver--she worked for the Geological Survey--and we went up to picnic or something at a cabin not far from Denver, and I was inquiring about this cabin. They said, "This is Forest Service land." I said I never heard about that.

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Leopold: I got back to Washington and went to the Forest Service and I said, "Please tell me, in all the states in the western states--Wyoming, Idaho, Montana, and on and on and on--tell me where there might be places where the Forest Service has summer home areas." They looked it up and said, "There's only one place now available, and that's at Pinedale, Wyoming." To my great surprise, Pinedale, Wyoming, was my back yard.

Lage: Is that Pole Creek, at Pinedale?

Leopold: That's where we camped. It was Pinedale.

Lage: What a coincidence.

Leopold: A great coincidence. So I got in an airplane and I went to Pinedale. I walked to the Forest Service office and I said, "May I see a map of the summer home area?" "Yes, here are the lots that are available." So I walked up in the country and I looked around at what was available. There were quite a few cabins there. I wanted the one farthest away, up against the forest so nobody could be my neighbor and nobody could be above me. So I went down and signed up for the lot. It cost \$35 a year.

So we were at the Forest office, and I said to Bill Emmett, my friend, my assistant, "At \$35 a year, why don't you get one?" So he said, "Okay, I'll split with you." We took two adjacent lots. We came back the next summer and we started to build.

Lage: When was this, by the way?

Leopold: This was in 1964. There's a long story about this, but the next year--it must have been the next year--I got a letter from the Forest Service saying, "If you don't build in your lot, we're going to take it away from you." I said, "What the hell? I've got a cabin." Bill Emmett said, "No, you built on my lot." [laughter] So I had built my cabin on his lot. I said, "The hell with that. We're going to build another cabin, but this time, I want an old log house."

So I went down to town. We spent so much time in the Highland Lumber Company there, where my friend was the owner, that they put in a telephone for me because I kept getting calls from Washington, and it interrupted their business. I went in to see my friend, the owner, and I said, "I want to buy a hundred-year-old log cabin, one room, the kind the settlers made." "Well," he said, "I think we can find you one. By the way, there's somebody right out here right now. Go out and talk to him." So I went out into the center part of the store, and here was this man I'd never met. I said, "I understand from Jim Harrower that you've got a log cabin you're about to burn down." He said, "Yes." I said, "Sir, let me go look at it before you burn it down. I'll give you \$50 for it if I can use it." He said, "That's fine."

I saw him about a year later, or two years later; I met him by chance. I said, "Do you remember me? I bought that log cabin from you." "Oh, yes, I remember you very well. I thought I gypped the hell out you." [laughs] I said, "I thought you gave me a great bargain." You couldn't buy one now for \$500.

So Bill and I went there and it had this much dirt on the roof. It was a dirt roof.

Lage: How far was it from where you--

Leopold: About twenty miles. We marked every log and we took the thing down. We hired a truck and we put all the logs in the truck. Then we drove up to the place where my lot was, and we built a cabin. Then later on, when Bill Emmett got married, his wife found this much too primitive. When I wasn't there Bill started to fix up the first cabin, and all of a sudden there was a bathtub in it. Okay, well, I went to Bill and said, "Look, I think it's time for us to split. I'll take the little cabin and you take the first cabin." So we did. He keeps it up just beautifully; everything is perfect. But it's kind of--. It's not quite to my taste.

Lage: This is the first one you built on his land?

Leopold: Yes. Our cabin is just one room with logs, and I built a fireplace in all of them.

Lage: So it's those original logs in the same plan that it had been?

Leopold: Yes. I enlarged the windows a little bit, that's all. But there was no stream, there was no river. There was no place to have horses and stuff. So I started looking for some land.

A rancher, Jim Noble, whom I didn't know at the time, came up to my cabin one day, and he said, "There's going to be a public meeting in which we're going to argue with the state engineer about water in this area, and I wonder if you'd speak on our behalf." I said yes, I would. Well, I made quite an impression on them, apparently. After I got to know Jim, they realized that nobody there could have done quite what I was able to do.

So Jim went to his father and he said, "I can't sell Luna any of my land because of my mortgage situation." But he said to his father, "You know what we need around here is a technically trained conservationist. Why don't you sell Luna some land?" And his father said, "Good." This was only a couple of months before his father died. It never would have happened if his father hadn't been there.

So Jim took me out some time later, and he said, "Let's go look at some land." So we went to this place and that place, and I was given a choice in this 3,000-acre ranch, I could take any damn place I wanted. So I said, "I want that place down there." He said, "That's fine." So we went to his father and said, "We found a place that you didn't mention, but maybe you'd sell this one to Luna." So anyhow, they sold me this little piece of property. So I'm in the middle of this very large ranch, you see. I have fourteen acres right on the creek.

Lage: And this is on Pole Creek?

Leopold: No, this is on the New Fork. Pole Creek is where we camped. New Fork is where we live now.

Lage: Are they near each other?

Leopold: They're about ten miles away. So now we have this big house, and now, when it came to that house, that was a little different. I went to Jim and I said, "I want to buy some old log cabins, and I have a plan of my own, but I need old logs." So Jim scratched his head and he finally said to me, "Yes, let's go see So-and-so." So we went, and I bought a pair of log cabins that were a hundred years old. With Jim's help, we moved all the logs down to my property. So my family built this house. It's a big house. In a week and a half, that's where we'll be. Barbara said this is the twentieth year that she's been in Pinedale.

Lage: Now, has this become an area where you've done a lot of research?

- Leopold: Yes. That's the whole reason why Pinedale was so important. Because of all the places I ever worked with John Miller, this place was one where we could reach all the different kinds of rivers that we wanted to within a short distance, and that's why we chose that area.
- Lage: So when you've gone up there for the twenty years, it's been a research center for you.
- Leopold: Well, certainly in the early years, yes. It's been more than this. We've had the house for twenty years. I've been going there for twenty, for thirty years, something like that. That's important, because to build a house of your own with your own hands makes a lot of difference. There, Barbara's children learned a lot and contributed a lot because the four of us really did it together.
- Lage: Okay, so they've been involved with it. And Barbara enjoys this kind of--
- Leopold: Oh, she loves it. Yes.

Since Retirement: Seminars in Hydrology

- Lage: We haven't talked too much about what you've done since retirement. I know there are a lot of things, but I was going to ask you about the Water, Science, and Technology Board.
- Leopold: Oh, that's not important.
- Lage: Is that not important?
- Leopold: No. I think what I'm doing right now for the Forest Service is probably much more important. I'm giving two courses this summer for the Forest Service personnel.
- Lage: Where do you give the courses?
- Leopold: Well, I've been teaching at Teton Science School in Jackson, Wyoming, every year for the last fifteen years, and I'm giving that up. But when I saw that the Forest Service was not sending the young people that helped us the most in the field to take our expensive course in Pagosa Springs, I said, "I'm going to give a free course just for them," so that's what I'm doing. I'm dividing this course between Teton Science School where I already have maps and a whole lot of things that I know about the area,

and then we're going to move to my front yard in Pinedale, and all the young people will camp outside in my yard, and we'll work on my river. So I spend half the time at Teton Science School and half the time down in Pinedale. This free course is a one-time deal.

Lage: These are Forest Service Personnel learning about hydrology?

Leopold: Yes.

Lage: And do they come from a variety of backgrounds?

Leopold: Last year we taught sixty people from all over the United States, yes. They came from every state in the Union, and they're going to send another thirty people to take the Pagosa Springs course in October again.

Lage: What is their technical background?

Leopold: Mostly fish. A lot of them are called hydrologists but don't know much hydrology. A lot of them come from fish and wildlife. We've had some people from range, and some from silviculture. But the kind of hydrology they're getting from us is somewhat different than what they're used to.

Lage: And the Forest Service is happy with this?

Leopold: They're very happy.

Lage: Are they open? I always had a stereotype of the Forest Service being kind of closed.

Leopold: Well, they were, but the thing is that this current court case has cost them so much money and they had to call in all these consultants like Dave Dawdy and me, people like that, they realize that they're very short of hydrologic talent. The top people began to see, "Look, we're just not up to snuff on this game." So I went to one of the chief people in Washington and said, "My suggestion to you is that you've got to build your staff. We've shown you that your people are way behind. Let me teach a course for them. I'll get Dave Rosgen, who formerly was with the Forest Service, and we'll teach a course together. I will teach the theoretical part primarily, and he'll teach the practical part." They said, "Fine."

So it cost them a lot of money. We had to give two courses, each lasting one week. They came from all over the United States. We put them up at a very nice condominium kind of a place not far from where we were going to work. We spent a half

a day in the office, in the lecture room, and then a half a day seeing things in the field. They were so delighted with it that practically all of the people said it was the best course they ever took in their life. So they wanted us to train some more people. I said I wouldn't do another set of two courses. We taught them back to back, and it was very stressful. We decided we'd teach one course, again lasting a week, the same place, Pagosa Springs. Pagosa Springs because Dave Rosgen has been doing river restoration work there, and we can take the students out and show them what actually can be done in the river.

Lage: Where is that? Pagosa Springs?

Leopold: Pagosa Springs. It's in southwestern Colorado. It's right where Rosgen lives.

"Ethos, Equity, and the Water Resource"

Lage: Your recent Abel Wolman Distinguished Lecture--can you tell how that happened to come about?

Leopold: Abel Wolman was a very famous water man, primarily in public health. He was M. Gordon Wolman's father, you see, one of my close friends. When he died, the Water Science and Technology Board, set up a lecture series in his honor. It was decided by the board to ask me to give the first lecture.

Lage: And how did you pick this particular topic? ["Ethos, Equity, and the Water Resource," published in Environment, volume 32, number 2 (March 1990)] It made quite an impact, it seems.

Leopold: Apparently so.

Lage: Are these things that had been troubling you?

Leopold: Yes, because with long government experience, I've seen the bureaucracy operating, I've seen the special interests that are so prevalent in what government does for us. The resource field in particular has been pressed by special interests, and therefore I want to say something about it. And I think this whole business that you saw yesterday of the NRA and the gun control bills, special interests are really pushing us around in a very serious manner.

Lage: You make a remark in that lecture about public servants being captured by the history of the organization. That's a very interesting concept.

Leopold: You see, the Corps of Engineers started out, as you know, being primarily concerned with large rivers and harbors. The Congress then expanded their work after the great flood in the Mississippi in 1936. That was the passage of the first flood control bill. The Corps of Engineers was given by Congress the responsibility of starting a much larger flood control program. As a result, therefore, we have the most expensive flood control project in the world on the Mississippi River, much more grandiose than anything that has ever been done elsewhere, but with costs that are not appreciated.

There are several kinds of costs. In the first place, the taxpayer paid for it but the benefits are not equally distributed. I guess I told you that when you build levees, you make your floods higher by confining the water. And the Corps, by the legislation, was paying 90 to 91 percent of the cost. And, then, the Corps expanded out into not only doing that but straightening rivers elsewhere.

So here, now, we've got the agricultural interests just in the state of California, the agricultural interests want more and more, you see. So that there's a heck of a lot of places in the resource field where we're being driven not by the social good, nor even by the economic good, but by special interests. Special interests, such as people who obtain monetary rewards from government projects, develop an historic tie to an agency. The Corps is an example. They get much congressional support from members who want money spent in their area. Now you see people trying to break out of this point of view, and it's doggone hard. It's very difficult.

Lage: The Forest Service certainly has this history.

Leopold: And the Forest Service, the same thing.

Lage: Did USGS have a constituency like that, a historic tie to special interests?

Leopold: Yes, in this respect: that the survey, through the cooperative program, has been doing the stream gauging for the states, in which the states pay 50 percent. But that's different. The states are paying 50 percent. In that regard, therefore, they have an interest, but these are not for individual people. Irrigation districts, cities, and the states need data for project design, and they pay the USGS to collect the data. It's

not what I would call special interest. Yes, it is a constituency, but the constituency, since they're paying half the bill, they're also saying to the survey, "We want gauging stations at these places because this is the place that we want the measurements made." The survey often says yes, and sometimes the survey will say, "I think it would be better if you did something else." But that's an entirely different constituency that I'm talking about.

Lage: I notice in this essay and in "The Alexandrian Equation," you had the classical references framing the essay. Have you done a lot of reading in classical history?

Leopold: Sort of, yes.

Lage: It puts such an interesting feel to the work.

Leopold: I think you always can catch attention, if you like, by doing something a little bit different, by referring to another example, not drawn necessarily from modern times.

Lage: And it makes it more universal somehow, too.

Leopold: Yes, I think so. In going to Europe--I went quite often when I was with the survey--I usually made it a point to concentrate on one thing. One year, for example, I read everything that I could about Michelangelo, and then I went to Rome and I didn't do anything but look at Michelangelo's work. Another time, I wanted to do just Napoleon, and another time I wanted to do Madame Stael. The ordinary tourist gets lost in the complexities of history, and I think it's much better to pick out something you're really interested in and do it in a more concentrated way. So that way, when I went to Greece a few years ago, I was really dealing only with Alexander, and it was very interesting.

Lage: And another time, you tracked down the Lunas in Spain.

Leopold: Yes. [laughter]

Lage: Okay, well, I feel as if we've come to a good place, and if we need to add something, we can do it after you've seen the whole.

Leopold: That's right, Ann.

Transcriber: Elizabeth Kim

Final Typist: Christopher DeRosa

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APPENDIX A

David R. Dawdy

LUNA LEOPOLD AS CHIEF HYDROLOGIST
OF U.S. GEOLOGICAL SURVEY

Interview Conducted by
Ann Lage
in 1991

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INTERVIEW HISTORY--by Ann Lage

During our extended discussions of his leadership of the U.S.G.S. Water Resources Division, Luna Leopold suggested that I speak also with David Dawdy. As a hydrologist who had worked for the Geological Survey before, during, and after Leopold's tenure as chief of the Water Resources Division, Dawdy had been well placed to observe the far-reaching and sometimes controversial changes he made to its program.

I met with Mr. Dawdy on May 3, 1991, in his San Francisco home. He spoke very candidly and directly about the changes instituted by Leopold and the reaction of an entrenched bureaucracy to a man who "turned things upside down," sometimes in a "shoot-from-the-hip" style that could alienate the "old-boy network." He also provided a valuable assessment of Leopold's importance to the science of hydrology, through his transformation of the Water Resources Division and contributions to university programs in hydrology, as well as through the impact of his own research and his application of science to public policy matters.

Mr. Dawdy reviewed the transcription of his interview session, making only a few minor corrections. He also donated for deposit in The Bancroft Library a tape recording of an interview he conducted with Luna Leopold on their colleague, Walter Langbein, whom Dawdy describes in these pages as "the only genius I have ever known."

Ann Lage
Interviewer/editor

January 26, 1993
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name DAVID RUSSELL DAWDYDate of birth JULY 1, 1926 Birthplace SAN ANTONIO, TEXASFather's full name PURL CORBEN WRIGHTOccupation _____ Birthplace CLARKTON, MO.Mother's full name GLADYS JOHNSTONOccupation _____ Birthplace CLARKTON, MO.Your spouse DORIS OSTRANDERYour children NONEWhere did you grow up? SAN ANTONIOPresent community SAN FRANCISCOEducation BA HISTORY TRINITY UNIV SAN ANTONIOMS STATISTICS STANFORDOccupation(s) HYDROLOGISTAreas of expertise SURFACE WATER, SEDIMENT TRANSPORT, STATISTICAL
HYDROLOGY, SYSTEMS ANALYSIS, WORTH OF DATA

Other interests or activities _____

Organizations in which you are active AGU, ASCE, AIH, IAHS, ASFPM

I DAWDY'S CAREER PATH IN THE GEOLOGICAL SURVEY

[Date of Interview: May 3, 1991]##

Field Assistant with a B.A. in History. 1951

Lage: Today is May 3, 1991, and I'm interviewing Dave Dawdy as part of the Luna Leopold oral history. Mr. Leopold suggested that I talk with you. I think he wanted to be sure that we got an understanding particularly of the tumultuousness of the period of his leadership in the USGS. So I hope that we can really speak freely. He thinks it's important that that story be recorded.

He mentioned that after he resigned as chief of the Water Resources Division that he was very isolated.

Dawdy: Oh, yes. That's quite true. But there was tumult during his reign also [laughter] because he turned things upside down. He changed the whole organization. That was his purpose when he came in.

Lage: Before we start, let's just learn a little bit about you, something about your background and when you came to the Geological Survey and that kind of thing, to give us a reference point.

Dawdy: Okay. I started off as a field assistant in the California district in '51, January of '51, right after the 1950 floods.

Lage: And what was your educational background for that?

Dawdy: I had a history degree. A bachelor of arts in history. I found out that, although I was working with a bunch of engineers, engineers are really afraid of mathematics.

Lage: That's interesting.

Dawdy: So because I wasn't afraid of mathematics, I became essentially the mathematician.

Lage: With your history degree?

Dawdy: With my history degree. [laughter] Well, I had taken a lot of mathematics when I was in college, in addition to history.

Lage: Where had you grown up and gone to college?

Dawdy: I was born and raised in San Antonio, Texas, and I ended up graduating from Trinity in San Antonio.

Lage: So then you became a field assistant for the Geological Survey in '51, did you say?

Dawdy: Yes, because we had a big flood in 1950, December of '50. Because of that, all of their people were out in the field. They didn't have anyone in the office so they went down on skid row and picked up some people to do things in the office to keep the records going, and I was one who got hired for working up their records. I was a field assistant but I wasn't really in the field; I was in the office. Everybody else was in the field. But over time, I ended up becoming an engineering aide, and then actually a hydraulic engineer.

Lage: I see.

Dawdy: The U.S. Geological Survey, the Water Resources Division, started setting up a research program in about 1955, '56.

Lage: That's about when Luna came in.

Dawdy: It was before Luna became the chief [of the Water Resources Division]. He was in the Geological Survey but he was not chief. He was in some sort of a branch. What did they call that thing? General hydrology branch or some such thing.

To Washington. on Flood Frequency Analysis

Dawdy: But anyway, because of my work on the '55 flood, a second big flood in California, I was the one who was actually in charge of all the office work for determining by indirect methods the floods of 1955. One of the people who was in from Washington to do the overall technical review was a guy named Manuel Benson. Ben was at that time being selected to start a research project in the Surface Water Branch on flood frequency analysis, unbeknownst to me.

But soon after, perhaps while this still was going on, Rolland Carter from Washington, who had been appointed to head up the research program in the Surface Water Branch, came through looking for people. And he interviewed several people, including me. I was the one who was picked to go back to Washington, I'm sure because of having worked with Manuel Benson on that flood. I worked in Washington as Manuel Benson's research assistant in developing the methods that are now presently used in flood frequency analysis in the Geological Survey. So-called state-space methods.

Lage: This is a mathematical--

Dawdy: Yes, you take all the data and treat it statistically to come up with a regional flood frequency analysis.

I went in to Washington in August of 1956 and was working on the flood frequency analysis for not quite two years. During that time, Luna came in as the chief. When the organization decided that I should move off and become a project chief of some sort, they took me off the flood frequency project and put me over in the office with Walter Langbein for a summer. At that time, Walter and Luna were working closely together. Luna had just become chief. And Tom Maddock was sharing the office with Walter Langbein. Tom Maddock was a new hire in the Geological Survey, but he had been Luna's first boss when Luna first got out of college. So I got to know Walter Langbein quite well, and Tom Maddock, and of course Luna.

Walter Langbein. a Genius

Lage: And what were your impressions?

Dawdy: My impressions?

Lage: Your first impression.

Dawdy: Walter Langbein was the only genius I have ever known. A really amazing person. He assigned me about six problems to think about during the summer, and we produced something like four papers. I'm sure that he knew the solution to all those problems he asked me about before we ever started, but he always sort of pretended to be naive; he wanted you to help him understand these things. It was a very, very stimulating summer, and after that was over with, I went back to the Surface Water Branch while they tried to figure out what to do with me.

Lage: Were you still in Washington?

Dawdy: Oh, this was in Washington, D.C., yes. It was all in Washington, D.C. The branches were all in Arlington, Virginia, whereas the headquarters was in Washington, D.C., in the old Interior Building, so I just moved across the river to the Surface Water Branch in Arlington. Rolland Carter and Walter Langbein and Luna were trying to figure out what project I should work on.

Papers on Sand Channel Streams

Dawdy: While I was waiting, there was a so-called Operations Research Project in the Surface Water Branch where they were trying to figure out how often to gauge streams. In other words, what was the accuracy trade-off in frequency of stream gauging? A guy by the name of Andy Anderson who had been district chief in Mississippi was in charge of this project. I started playing around with his data and came up with an interesting relationship that I thought was quite different. I'd never seen-- Well, I had seen it before too, but it was very remarkable in this particular stream in that it was a large stream that went from very small flows to very high flows. It was the San Juan River at Shiprock [New Mexico] and because it had a snow-melt component it would get up to high flows and stay there; and because it shifted around, they had a person who sat there and measured it every day. That's why they picked this to study, because they had so many discharge measurements.

Lage: To see how frequently it was necessary to measure?

Dawdy: Yes. Whether they can get it equally accurately with fewer measurements. Well, I found out that the shifting of the stream could be explained and took this thing and showed it to Rolland Carter, who got very excited because the people in our research group in Colorado State University had been getting these same sorts of results in the flume, but everyone was trying to explain it as a flume effect, rather than a real physical effect.

Lage: When you talk about the shifting of the river, do you mean the river bottom?

Dawdy: The bottom. The bottom goes up and down, so therefore when you plot-- The way you usually get discharge measurements is to measure periodically and then plot the stage, the elevation of the water, against the discharge, and you get a relationship so you can

determine the discharge at any time. But if the bottom keeps going up and down, it's very difficult to get that relationship.

So what I did was very exciting to Rolland Carter. He took me over immediately and we had a meeting with Luna Leopold, and they laid this out in front of Luna, and Luna got very excited about it. The result was that I was shipped off to the western United States for six weeks to go around looking for other sites where this occurred. Before I went there, I took a bunch of stations and worked up their ratings to check and see whether I thought that they would fit this relationship, and then I went all over the West visiting those sites and looking at them and getting bed material samples.

Lage: Did you have a team to go with you?

Dawdy: No. The only team was my wife. Doris went. One of the funny stories on that was that I was just shipped out with a general travel authorization. When I got to Denver, I went and asked for an automobile to go around for six weeks. They said finally, "What's your charge number?" And of course, I had never heard of this so I didn't know what they were talking about. So I said, "What charge number?" They said, "It's a seven-digit number." I said, "Oh, that." I gave them seven digits, and they wrote them down. He had given me enough of a hint so at least I had a rational number. It turned out that it was a balancing account in the director's office, meaning that it was one of those accounts where they transferred money in and out temporarily. So there were all these transactions that nobody knew why they were going in and out. So this charge on this car was lost forever. Nobody ever asked why the car got into that account. [laughter]

Anyway, I went around doing this analysis and ended up writing a water supply paper on that, which became the basis for the way resistance to flow in sand channel streams is done. Now it is written up in the U.S.G.S. procedures [USGS Water Supply Paper, 1498-C].

And at about that same time, I got involved with the Albuquerque district on analysis of all their data on the middle Rio Grande, because they'd been collecting a lot of data and they'd tried to write a report which had really bounced, and very badly, and they were in some deep trouble over this. And I, on this trip when I went around, passed through Albuquerque and ran into the people that were working on this report. Jim Culbertson was the senior author on the report and, in fact, the only one left. The other author had left the survey over this report.

Lage: Because of having so much difficulty with it?

Dawdy: Yes. It bounced, and there had been so much repercussion I guess he just said, "To hell with it." But anyway, I didn't know all this background, so when I saw the stuff, I naively volunteered to help out. This got great approval in Washington because Luna very much wanted these data to be analyzed, so I ended up spending some time in Albuquerque and in Santa Fe, working with the local people on working up their data, that ended up in another water supply paper [USGS Water Supply Paper, 1498-F].

Lage: So were you able to figure out what the problem was then?

Dawdy: Oh, yes. It was that they hadn't really known how to look at the data. It was sort of the same approach that I was developing as a result of this paper that ended up in front of Luna before I went on the trip, so I was enthusiastic about looking at their data in that light. And it turned out, of course, all these were sand channel streams and they all fit that general relationship.

Lage: So this really explained a great deal.

Dawdy: Yes, quite a bit.

Well, anyway, this got me started as one of the researchers in sand channel streams and in sediment transport in general, and therefore brought me to some attention with Luna and Walter and the whole crew.

Lage: Luna seems to appreciate that kind of scientific advance, new ways of looking at things.

Dawdy: That's right. He likes to see people who can understand how things operate, or think about how things operate, how the system works. I worked on sand channel streams for a couple of years out of Washington. And then they were beginning to think in terms of computers. The Groundwater Branch was pushing at that time something called analog computers.

Lage: This was the early days of computers?

Dawdy: Yes, before digital computers took over. And I was shipped off to Phoenix, Arizona, to this ground water lab to investigate the utility of using analog computers in surface water work. Well, there was a lot of utility in that, with lots of potential, but because the digital computers took over very quickly, it became obvious that it was much cheaper and easier to use digital computers, so that whole area of effort, area of research, sort of died.

Lage: There was no transference between what you developed for an analog computer and--

Dawdy: An analog computer used resistors and capacitors to build the equation and solved it all simultaneously and rapidly. The digital computer takes that equation and turns it into a mathematical iterative solution. So it's a step removed from the analog computers but it gives you more precision, and because of the advances in speed and everything that have taken place in digital computers, they very rapidly overtook analogs, because almost anyone can learn to use a digital computer, whereas it took a little more knowledge to use the analogs.

Lage: Had you still done all this with just your history degree?

Dawdy: Yes.

Lage: So this was all on-the-job training?

Dawdy: Yes. All of my hydrology has been on the job. On-the-job training. And all of my research was also on the job.

Higher Degree in Statistics through the Government Training Act

Lage: And your mathematics?

Dawdy: Well, when I was in Washington, because of all of this, I started taking math in graduate school at the American University. I was fairly well toward a master's degree in mathematics when I left Washington and went to Phoenix. I was in Phoenix about nine months, and Roy Hendricks came through and told me--asked me, depending on which way you want to put it--to go up to Stanford and get a master's degree in statistics.

Lage: Now, who is Roy Hendricks?

Dawdy: Roy Hendricks was the associate chief under Luna, and he was the one who became the chief hydrologist after Luna. So he was in on all that turmoil.

So I went off to Stanford under the Government Training Act. I think I was one of the first ones to go under the training act.

II LUNA LEOPOLD AS CHIEF HYDROLOGIST, USGS, 1957-1966

Hiring Ph.D.'s

- Dawdy: Luna jumped on the concept of the Government Training Act to start in-house training for his people. One of the first things that Luna did when he came in was to start--well, he did several things. He reorganized the whole administrative outfit. He started stressing the research program. We had started a research program within the survey in the Water Resources Division before Luna became chief, although he may have been involved in the thinking behind getting it started, before he was chief. But Luna emphasized the research program much more, as a support for the program as a whole, and he started hiring Ph.D.'s. In fact, it became almost impossible to hire anything but a Ph.D. for a while.
- Lage: And that was a new development?
- Dawdy: Very new. There were a few Ph.D.'s when I went into the research group. To the best of my knowledge, there were no Ph.D.'s in the Surface Water Branch. There were several people with master's degrees. A guy by the name of Nick Matalas was one of the first ones who was hired as a Ph.D., but I think he was hired after Luna became chief, and Nick was put in our research group.
- Lage: Was this something that was resisted, the hiring of Ph.D.'s? Did it make people feel threatened?
- Dawdy: Yes. Most of the people were involved in the data collection program. This was stressing the research program over the data program, and the limitation on hires put a lot of pressure on the districts to try to keep the data program going, and they felt that Luna was hiring all these Ph.D.'s that were not helping them getting the daily work done.
- Lage: At the same time--I'm just guessing from what you said--was the division thinking of ways of reducing the number of times they had

to collect the data, or was that not connected to a reduction in data collection work force?

Dawdy: That was not connected. That was generally just trying to do a more efficient job. They did things like, how accurate does a discharge measurement have to be, how many times do you have to measure a stream in order to gain a certain level of accuracy on the annual record?

Lage: So some of the research went towards making the data collection program more efficient and more accurate.

Dawdy: Yes, that's correct.

Administrative Reorganization and the Old-Boy Network

Lage: We're getting at the sources of some of the tension here.

Dawdy: Part of the main tension within the district program was that Luna upset all the old-boy network. He came in and started stressing that the way you become a district chief is to have a publication record; you have to have done something besides just rise up through the ranks by running the data program. He, in addition, started taking people from district A and making them chief in district B, whereas in many of the districts there was an heir apparent, and the district chief had complete control over who was going to get promoted and who was going to follow him.

Lage: So the districts had been their own little fiefdoms, then.

Dawdy: That's correct. Very much so. The district chiefs were gods. That was changed when Luna came in, and there was lots of resistance to that.

In addition, Luna took the branches at the field level and combined them. We had three operating branches: the Surface Water Branch, which gauged streams; the Groundwater Branch, which went out and measured wells and determined how much groundwater there was; and the Quality Water Branch, which measured the chemical constituents in the water. These three were completely independent and quite often didn't talk to each other.

Lage: And they had separate offices?

Dawdy: They might have offices right next to each other, but they weren't allowed to talk to each other in some cases.

I remember when I was involved in--. After I'd gone to Washington, I guess probably while I was in Phoenix, the state of Arizona asked the USGS to design a program. They had some problem; I forget what it was. But I sat in on this planning session, and each of the district chiefs--there were three district chiefs: the groundwater, surface water, and quality water--sat in on it. They all went back to design a program. It was very funny because the state told them how much money was available, and they came in with three programs. The Surface Water Branch came in spending all the money on stream gauges. The Groundwater Branch came in with all of the money being put into drilling wells and collecting groundwater data. The quality water guy came in measuring chemical quality all over the state. There was no way that they could compromise on this thing; there's no one to tell them to coordinate this.

So anyway, Luna, seeing problems like that, combined the three branches and made a district chief rather than a district chemist, a district geologist, and district engineer. There was just a district chief over all three branches, and that eliminated a lot of positions. A lot of people who were in line to become district chief or district engineer or district chemist or district geologist were now eliminated from consideration. And a lot of the people that thought they had a sure idea of what their future was, suddenly didn't have, and this created some uncertainty.

Lage: Did it also threaten their having a job at all, or were they--

Dawdy: No, no, but quite often they were not the jobs that they thought they were going to get. They weren't the controlling jobs; they were staff jobs rather than line jobs. Also, quite often they were asked to transfer. Whereas they were expecting to inherit a particular state, they were asked to move off and be a minor position in some other state, which quite often they didn't like.

Lage: Was this done as a way to ease people out, do you think, or just for more efficiency?

Dawdy: Partly, I guess, but mainly I think it was that Luna saw the need for coordinating the programs in the organization. You couldn't have these people competing in every state and not being able to build an integrated program that solved the problem. So in order to do that, you had to put all these people together and make them talk to each other.

Lage: I may be not understanding the organization completely, but how did these district organizational programs fit together with the data collection organization?

Dawdy: They all collected data.

Lage: So they were all part of data collection?

Dawdy: And they all did interpretive reports. For instance, in the Surface Water Branch you would have a data section that took care of all the stream gauging. And you would have an analytic group that might be called the hydrologic unit, and they might have a hydraulic unit. They solved different sorts of problems, but they would do analytic reports.

Lage: But they were all organized through this district system?

Dawdy: They were all through the surface water district chief, who reported to the surface water chief in Washington, who then reported to Luna. In the Groundwater Branch, you would have a similar thing. You'd have a data unit which went out and measured all the wells, and you would have an analytic unit that did the groundwater analysis for the different basins. They would report to a district geologist, who would report to the chief of the Groundwater Branch, who would report to Luna. The Quality Water Branch is the same way. They'd have a group of people who'd go out and collect the data, a group of people who would do reports, who report to the district chemists, who reports to the chief of the Quality Water Branch, who reports to Luna. But the only person who could bring all these together would be the chief, Luna, and he couldn't solve all these problems at the district level.

And prior to when Luna came in--. Although, some time around the early fifties, the need for this integration started becoming apparent.

Lage: It wasn't only Luna who observed--

Dawdy: I think that everyone realized some of the problems. It's just that Luna saw that the solution was changing the whole organizational structure. He made the branches so-called staff branches rather than supervisory, and took the management away from the branches and set up a separate organization for managing the districts.

Lage: Was it integrated on the district level?

Dawdy: Yes, it was integrated on the district level, and then all district chiefs reported to regional hydrologists who could coordinate a regional program, so sometimes you could even get states to talk to each other.

Lage: What about the research branch? Was that ordered by districts also?

Dawdy: No, that came up entirely separately, and it was organized originally at the regional level. Each of the regional offices had a research group who reported to a-- . Well, it started off--let's step back to before Luna came in. The research program was organized along the branch lines, and the research people were mostly in Washington or in Denver.

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Dawdy: They reported to the branch structure. For example, I was in the research section of surface water, and our chief, Rolland Carter, reported to the chief of surface water, and then he reported to Luna. So the research people were pretty well unrelated. The sort of interesting thing was that I was in the Surface Water Branch, but I got very much involved in sediment transport, and this was quality water. It created some consternation, because when I went to Albuquerque I worked for the Quality Water Branch on their report on the Rio Grande. The realization within the Surface Water Branch that the sediment had something to do with their game, which was resistance to flow, was a very radical idea. So the dichotomy between branches went over into the technical end of the game.

Lage: I see that. It kept them from looking at the problem as a whole.

Dawdy: That's correct. In fact, when I got started in this sand channel stream stuff, there was a file cabinet in the Surface Water Branch hydraulic section which handled resistance to flow, and they had a group of stations for indirect determinations which they had segregated, and the reason for this was because they were sand channel streams, and the resistance to flow was much too low, so therefore the Surface Water Branch would not accept those numbers. And even though they measured the discharge and computed the resistance to flow, so therefore that was what it was, they had notations on there that these numbers could be used for that discharge, because they had the discharge measurement with the current meter, but they could not be used for any other purpose. I sort of died laughing over that, spread the word around [laughs], which probably wasn't appreciated very much, even by Rolland Carter, who was a good, loyal surface water man at that time.

Reeducating the Old-Line Staff: Rolland Carter

Dawdy: It was very interesting, because Rolland Carter was reeducated by Luna. This was sort of one of the symptoms, or whatever you want to call it, of Luna's approach. Rolland Carter was a very sharp guy, one of the smartest people in the organization.

Lage: And he'd been there for a while.

Dawdy: Oh, yes, he was an old-line technical person in the organization. He was about, I guess, ten years senior to me, and he was the chief for our research group in surface water. He had gotten his master's degree at Georgia Tech, and he had gotten two gold medals for papers from the ASCE [American Society of Civil Engineers] so he was really a very sharp person.

But he very much was not interested in sand channel streams and the difference in how to approach hydraulics. The hydraulics of sand channel streams and movable boundary streams was quite different from rigid boundary hydraulics, and Carter, along with everyone else in the Surface Water Branch, were very oriented toward rigid boundary hydraulics, and that's what our research group at Georgia Tech was studying--rigid boundary flumes. We had a small research group doing hydraulic research at Georgia Tech from the early fifties.

Lage: But they were Geological Survey people?

Dawdy: Yes, they were Surface Water Branch people who reported to the chief of surface water in Washington. When we finally set up a research section, they then reported to Rolland Carter, who was the head of the research section.

Luna, seeing this in Rolland, and after several knock-down, drag-out discussions, which we had quite often with Luna around on technical matters--he periodically would call meetings and get everybody together to discuss particularly sediment transport and hydraulics and resistance to flow in sand channel streams.

Finally, he took Rolland Carter, removed him from chief of the research section, and put him in a small room. It was very funny because it was a small room with a great, big, square column in the middle of it, so that Rolland's desk was right up against that column and there was hardly any room to get around in there. Rolland was given the job of coming up with a planning document for research in sediment transport, because they'd had so many arguments that Luna said, "The only way to educate Carter is to make him tell the organization what the research problems are."

So Carter spent about three or four months just reviewing all the literature describing what all the problems were, what wasn't known, what was known.

Lage: In this one area? Sediment transport?

Dawdy: Yes, that one area. He came up with a really magnificent summary which should have been published and distributed to all researchers everywhere, but it was just internal. The main purpose of it--and Luna didn't care what the damn thing said [laughs]--he wanted Rolland Carter to find out what the problems were, and he did.

Lage: So he was reeducated through this process?

Dawdy: Oh, completely. Rolland was completely reeducated by that. He became a very knowledgeable sediment transport person.

Lage: And he was willing to look at the movable boundary streams as well as the rigid--

Dawdy: Yes, look at the movable boundary streams and, of course, to bring in all this knowledge that he had of the hydraulics, which was based on the rigid boundary knowledge.

Lage: Was his background as a hydraulic engineer?

Dawdy: Yes. I was going to say Georgia Tech. I know he got his master's at Georgia Tech, but I don't know where he originally came from. Somewhere down south. In the Surface Water Branch, most of the leadership in the forties and early fifties was Southerners. Mississippi, Alabama, and Georgia.

Lage: Was there a reason for that?

Dawdy: They were much more cooperative with each other. They organized sort of a regional exchange of information, and they transferred among districts quite often, and they helped each other out. So they sort of became the technical leaders in the Surface Water Branch, where Rolland Carter came from. That's where our branch chief, who was Melvin Williams, came from. He came from Alabama. Carter came from Georgia. This Andy Anderson I was talking about came from Mississippi. Even though he was a Swede from Minnesota, he was a good Southerner.

Luna's Shoot-from-the-Hip Style

Dawdy: The way that Luna worked was to sort of shoot from the hip, to an extent. He was known for shooting from the hip quite often, and sometimes that worked out well, and sometimes that didn't work out so well.

Lage: Do you have an example there? He was following his instincts, do you think?

Dawdy: Yes, he would talk to someone in the field, and they would--. Everyone tried to influence the chief, so they would get his ear and explain how they had the solution to something. Luna would say, "Fine," and he would pick them out and say, "There. That's solved." Quite often, they could talk better than they could do, and they would end up not living up to the expectations, because Luna had very high expectations. If he thought you could solve the problem, you'd damn well better solve it.

Lage: So then what would be the consequences?

Dawdy: The consequence was that people would be pulled out of what they were doing a good job in and put into a situation they couldn't handle, and then they would be shunted aside, and their career would be at a dead end. And there were several examples of that.

And there were other sorts of things too. Luna very much wanted to change the way the organization operated. I remember, for example, one case that everyone was quite upset about was this Andy Anderson, who had been district engineer in Mississippi, moved into Washington and did this operation research study, and then he was moved up to be an assistant chief hydrologist under Luna. He was sort of the administrative head of the organization.

Luna, in those days, as always, liked to keep his hand in doing field work. He thought that every technical person should be a manager during part of his career, and every manager should keep his technical competence.

Lage: Was that well accepted, that idea?

Dawdy: It was well resisted. [laughs] Luna went off into the field each summer to do various research projects he was interested in. One summer he went off, and he left very definite instructions about some decisions being made right at the end of the fiscal year. At the end of the fiscal year there was always chaos, because money is coming and going, and all sorts of decisions have to be made. Luna was gone, and Andy Anderson was left in charge. Luna had given

very specific instructions on one item; I don't remember what it was, but I know that when this decision came up, he disagreed with Luna's decision, so he made his own decision. When Luna came back, he went through the ceiling and just forthwith removed Andy from his position. He at that point was acting assistant chief hydrologist, and he ceased being acting much of anything. This upset the troops, the old guard.

Lage: Was Andy Anderson popular with the old guard?

Dawdy: Oh, yes, he was popular. He was a real nice guy, lots of friends, and as I say, he was one of these people who had helped keep this Southern group organized and doing things and all. He was a doer.

Lage: And you don't remember what the decision was?

Dawdy: No. It had to do with some millions of dollars that something was supposed to be done with it. And Andy did something else with it. I forget exactly what the difference in the decision was, but I know it was sufficient so that Luna removed him. In later discussion, when everybody was discussing this, my feeling was, "Wasn't he told? Didn't he disobey?"

Lage: He was insubordinate.

Dawdy: He was insubordinate, yes. What would you do if your people did that? But the trouble was, see, the good old boy network didn't like it to happen to one of their favorites. And he was a good friend of mine. He still stayed around Washington and did staff work, but he was no longer as influential as he was.

And there were several other people that ended up in similar sorts of situations where Luna would make firm decisions and carry them out regardless of consequences, and particularly regardless of his particular popularity in doing it. He felt that it had to be done, so he did it. And that wasn't the way the organization worked. The organization was very paternalistic, and that was quite different. As I say, there were a lot of people who were waiting to inherit positions that they had worked to achieve for twenty years, and then suddenly Luna decided they should be in Timbuktu instead of Kalamazoo.

Lage: He mentioned, without mentioning a name, but a particular case where he wanted somebody to move, and insisted that they move, and then when he left the chief's office, it was all shifted. The person was not moved or was brought back, and the policy about moving was changed.

Resistance to Change in the Bureaucracy

- Dawdy: That's probably true. Many of Luna's policies were sort of rescinded after Roy Hendricks took over, because Roy was part of the good old boy network. He came out of the South, and he was part of that crew. He very much didn't want to rock the boat.
- Lage: But he was associate chief under Luna. How did he work that closely with him, and--
- Dawdy: As long as Luna was in charge, he was a good second man and did what he was told. It was only when he saw that Luna's position was in difficulty that I think that he started thinking about--what would you say?--building friendships for the future. [laughter] And that didn't include Luna, because he could see Luna was going out.
- I was at the meeting in Columbus, the district chiefs' conference in Columbus, where all this interplay was going on, where everybody but me knew what in the hell was going on. I didn't know. I knew something was going on.
- Lage: Now, when would that have been?
- Dawdy: That was just before Luna stepped down.
- Lage: Were the district chiefs kind of mounting a rebellion?
- Dawdy: No. Luna didn't get removed because of the district chiefs. Luna didn't see eye to eye with the guy who came in as director, a guy named Pecora, and I think that was the biggest reason he stepped down. But the word was out that Luna was of limited tenure when this meeting took place in Columbus, so there was lots of jockeying around, and that's when--. I forget now even what the argument was that took place, but I know that Rolland Carter and Melvin Williams--one was the chief of the Surface Water Branch and the other was the chief of the research section under him--stood up and said opposite things at this meeting and almost wouldn't speak to each other afterward; I know because I was in the corridor [laughter] after the meeting, and Rolland was just livid with rage. He thought that they had agreed on whatever the hell it was.
- Lage: Was this argument related to Luna's stepping down?
- Dawdy: No. It was related to how the division should be run.

So anyway, the district chiefs were all trying to figure out what was going on and how they could fit into the old boy network and how they could reestablish it under Roy, which they did.

Lage: Because the district chiefs were the creatures of Luna.

Dawdy: That's right, but they all were people who had been in the organization before Luna. Many of the district chiefs were very anti-Luna, even though they benefited from Luna's legacy.

When I got back to California, there was still grumbling about Luna and what he had done. This was long after Luna was gone, and Lee Peterson was the district chief. I was the assistant district chief. We were all sitting over lunch one day and discussing things, and he was talking about Luna and his program. I said, "You're district chief. Do you believe we should go back to three districts?" "No." I said, "Well, then you're in favor of that." "Yes." I said, "We did it! But you wouldn't change it, would you?" "No." I said, "Would you tell me anything that Luna did that you would change? What is it that--?" Well, he couldn't really think of anything in particular. The problem was that Luna changed the way things were done. He upset everything.

Lage: It's a great study of change in a bureaucratic organization.

Dawdy: Oh, yes, quite. And almost everyone resisted. But everyone agreed that what was being done was being done for the better. That was the odd part about it. And it was done so slowly--not because Luna wanted to do it slowly, apparently, but it took something like four years to switch them from the three branches to the district chief set-up, I understand mainly because Nolan didn't want to put up with all the ruckus all at once.

Lage: But Nolan, apparently, was in favor of all of this.

Dawdy: Yes, he was in favor. He very much wanted Luna to do things, but then when Luna started doing them, apparently he wasn't interested in doing them too fast. He didn't object to them getting done; he just didn't like the ruckus that was generated from Luna doing what he wanted done.

Lage: Were there other major things, in addition to the change in the district set-up, that were disturbing?

Dawdy: Yes. The whole research game, the whole idea that the district chiefs or any administrator should be judged by their scientific skills. The other thing that Luna pushed which was resisted by the researchers was he tried to get the researchers to move in and take administrative positions. He essentially said, unless the

researchers move in and run the organization, take their turn, they're going to be run by these people who don't understand research. Everybody agreed to that, but none of them wanted to go in and do that job. In fact, one person that I particularly remember was a guy named Stan Schumm, who was also in quantitative geomorphology, as it's called, which is the area that Luna was most interested in. Luna tried to get him to transfer into Washington to take over some position for a period of time, promising that it would be a limited time. Stan, rather than do that, quit and went off to Colorado State University on the faculty, and is still there.

A Permanent Change in the Orientation of the Water Resources Division

Dawdy: The general things that Luna changed was the whole orientation of the organization. If you looked at the Water Resources Division today, the things that Luna did, still, because they achieved a life of their own, are still there. Nobody wants to go back. The research character is still very strong and they are still hiring good people.

Lage: They still have Ph.D.'s?

Dawdy: Oh, yes, they still stress the Ph.D.'s. They've got probably as good a research group as there is in any one country or in the world, as far as that goes. That came about purely because of Luna, and with lots of resistance. But Luna for a period of time emphasized Ph.D.'s for research, and he emphasized people at the district level getting their master's degrees, going back, and he stressed the government training program.

In fact, after that was under way for a couple of years, he came around discussing it. When he came to Menlo Park where I was at that time doing research, two of us voiced some complaints. As Luna usually did--this was his "shoot from the hip" thing I was talking about--when we disagreed with what he was saying--he was painting a rosy picture and we said it wasn't as rosy as he was painting it--he said, "All right. You, Dave, and you, Ivan"--Ivan Barnes being the other person--"I appoint you a committee of two to review this whole program and tell me what's wrong." Which we did. We spent a couple of months looking at all the applications, what had happened to them.

Lage: Now, this is a program for further education?

Dawdy: Yes, that sent people back to graduate school. GS [Geological Survey] paid people to go back to graduate school. When I was at Stanford, I was on full salary, full tuition paid, all books paid, anything I wanted was paid for.

Lage: And what did you find from your survey?

Dawdy: We found out that what we'd said was correct.

Lage: Oh, really?

Dawdy: Yes. Many of the districts were not in favor of this, and we pointed out that there were certain district chiefs who had turned down every application. No matter who applied, they said no. And other district chiefs who believed in it always said yes. The success of people getting into graduate school once they got there didn't have much to do with the recommendations of the district chief, and yet it was going through the district chief.

Lage: I see. So you weren't objecting to people going back to graduate school, but to the way it was administered.

Dawdy: No, we were objecting to the way it was administered, yes. So Luna then took the program and put it under a national coordinator, with a little more control at the national level.

Lage: So that people would apply not through their district chief?

Dawdy: They still had to go through the district chief. You always have to go through your own administrator, but the people were chosen less on the recommendation of the district personnel and more on looking at the person and what he had accomplished and what he wanted to do.

Lage: Now, how did your experience of going back to school work out? Luna made some reference to that. They wanted you to take a class in something that you'd practically written a book on.

Dawdy: Not quite. I was sent back to get a master's degree in statistics, and I had worked a lot on statistics, but not nearly the way that Stanford teaches statistics. So I went back and had to run real hard with those young punks to get the master's degree in statistics. But it turned out to be a good pay-off. I resisted. I told them that what they should do is go hire a statistician, but they said no, what they really needed was a hydrologist to learn statistics, and that was easier than getting a statistician and teaching him hydrology. And I think that was true. It turned out that way. The hydrologic background was very important.

Building Programs: Looking at Systems and Processes

Dawdy: Let's see. What other sorts of--? Luna was very influential in building certain programs within the survey. He of course was personally interested in quantitative geomorphology. He pushed the sediment program very much. The sediment program is almost defunct now in the Water Resources Division.

It's very interesting because nobody really thought in terms of systems. I'm now involved in a National Academy [of Sciences] committee--National Research Council--on Glen Canyon environmental studies. One of the interesting things is that when Glen Canyon Dam was built, they discontinued almost all the data collection on the Grand Canyon.

Lage: Just at the time when they should have been following--

Dawdy: Just at the time when they should have been emphasizing it, they eliminated it.

Lage: The survey did?

Dawdy: The survey. Because the Bureau was paying for it--

Lage: The Bureau of Reclamation.

Dawdy: --and the Bureau didn't want it. And the survey didn't have enough insight to stress that they needed to know what was going on in order to study the result of the dam downstream from the Grand Canyon.

Lage: That's incredible.

Dawdy: So now they're going in on a real intensive ten-year study the Geological Survey is proposing to measure sediment movement through the canyon. It's the sort of thing Luna would have said had to be done if he had been in. In fact, before the dam was closed, Luna organized a trip through the canyon to get a background state of the canyon just before the dam was closed, because he thought it should be done. That way of looking at things in terms of process just hasn't been in existence in the leadership of the division since Luna left.

Lage: So the leadership in choice of research projects--significant research projects--is this one of Luna's contributions?

Dawdy: No, not that so much. The research program has built up a life of its own from the organization. When Luna started off the research program, what he did was to hire people and sort of assume that if you hired the right people, that they would choose the right problems, and you let them prove themselves.

Lage: But he didn't choose or encourage certain problems. He must have in some way.

Dawdy: Oh, he did, in the sense of how he hired people, yes.

Lage: In which field he hired people?

Dawdy: Yes. If you hire a geochemist, he's going to study geochemistry or something. And if you hire a quantitative geomorphologist, he's going to study quantitative geomorphology. So in that sense, Luna picked the areas by picking people, but he didn't manage the program in the general sense. He got a group of people together and he had a regional research hydrologist who sort of did supervisory stuff but didn't really micro-manage the research program.

Reorganizing the Research Unit in the 1970s

Dawdy: This was a necessary stage for research. After Luna was gone a few years later, they started organizing the research program, trying to figure out who was doing what and why, and trying to judge the people in terms of their productivity. So this structure was set up within the organization and managed through the regions, and now, apparently, has become independent of the regions and is managed separately.

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Dawdy: So research has become an integral part of the organization. It is sort of independent of the chief hydrologist that happens to be around at the time. The first to follow Luna really didn't do very much in the sense of changing the research program. It just continued, and they had so many slots, and when somebody moved out, somebody was hired to come in.

Lage: In that same field?

Dawdy: In that same field, and it sort of went on by itself. And then, sometime just before I left the organization, which would be in '75, we set up this structure or procedure to sort of manage

research. They started trying to figure out who was doing what and how they fit into the overall program of the survey, and tried to influence people into different directions, and tried to set up priorities for who to hire and what skills and such.

That has been set up in such a structure that now the chiefs who came in later--or the only one; there's only been one more--is presented with a structure which he accepts, and the research program carries on through inertia. [laughter] So I think even though there's pretty low morale in the Water Resources Division these days for various reasons, that the research program that Luna instituted is still a strong component of the organization.

Lage: That continues, but people at the top don't necessarily have the understanding and enthusiasm that he brought?

Dawdy: Well, they don't have the understanding or enthusiasm, and they don't direct it very much. It's sort of self-directing, in that they do appoint an assistant chief who is over the research program. These people generally come out of the research group, so they sort of keep things going along.

Lage: Are people assigned to research tasks, or do they have the ability to pick their own area?

Dawdy: Generally speaking, they pretty well pick their own area, at least when I was in. I think it's still that way. Periodically, you wrote a proposal saying what you were going to do and how you were going to do it and requesting funding. These then were judged at the regional level. This was transferred over to this management group--what do they call it--deputy assistant chief of research, who had under him these research advisors, and all of these people together came up with assessment priorities in establishing budgets and that sort of stuff. So the only thing the chief sees is the total budget for research, which he can then say, well, it should go up 5 percent or down 5 percent. Because everything is done marginally. It's never done--

Lage: But nobody says we really need to be studying the effect of dams on the Grand Canyon.

Dawdy: In general, no.

Lage: That would be a public policy issue.

Dawdy: It's a little specific. What I was thinking more there is saying that there would be processes happening in the Grand Canyon where you should be studying that process.

Lage: Not for political reasons.

Dawdy: Not particularly. Just because it's going to change, which it has. Now we know it's changed. Now we're trying to figure out how to explain it. So they're going in with a big thrust now.

It seems to me that they're more liable to look for short-term gains, but that was not particular to the people who followed Luna, who sometimes wanted things done overnight too. It's a characteristic of the man on top. He always thinks that things can be done immediately. But quite often, the people in Washington who are running the organization will run into a congressman or a senator or the secretary of the Interior or something, who wants something and they promise on the spur of the moment, and then the poor people down at the bottom have to produce. Sometimes they can't produce.

A good example of that was this Kesterson problem that occurred in the Central Valley, where apparently the chief hydrologist was under a lot of pressure politically, so he quite often promised things because he thought they could be done. Sometimes they could be done, but they couldn't be done in the framework that he had to produce them in.

Pink Terror Memos

Lage: I heard reference to the Pink Terror memos.

Dawdy: Oh, yes.

Lage: What were those?

Dawdy: When Luna came in, he wanted to put forward some of these ideas we were talking about, about professionalism in the organization and about how people should keep up their technical competence and all. So anyway, he came out with policy memo number one, which was a big joke in the organization because it was distributed on pink paper, and that was the Pink Terror. [laughter] And everyone joked that it was about time that they finally had a policy in the organization that had been around for eighty years without one; it was about time they had a policy.

But anyway, if you re-read that today, it's still very appropriate. In fact, it's still in effect, I understand.

Lage: What was it in reference to?

Dawdy: It was in reference to the professionalism in the organization and how the organization should conduct itself, and how everyone should be expected to keep technically competent in his field.

Leopold's Contributions to the Publications Program

Lage: Did this include administrators continuing their research and doing publications?

Dawdy: Yes, stuff like that. And then another interesting thing was, there was a big backlog of publications, and Luna said his first priority was to eliminate that backlog. So he came to the different branches and asked them how many papers they had in process, how much it would cost to get them published, and he gave them a deadline: you will publish all of these within a certain period of time. Many of those papers shouldn't have been published. [laughs]

Lage: Were these interpretive papers or research papers?

Dawdy: They were mainly interpretive papers. Not research papers. They pre-dated the big research program. They were techniques papers. For instance, each of the branches had their techniques for doing their work in the field. Luna said, "These will be published." So everyone had to either develop one or publish it. What resulted from that was a great big series of papers on techniques in water resources investigations, which finally forced the organization to put down on paper how they did their work. They'd been going along sort of ad hoc.

Lage: Now, when you say that some of them shouldn't have been published--

Dawdy: There were some papers in these techniques manuals which were in the process of being outdated by the research program which was just getting under way. Luna said, "Is that how you're doing it today?" "Yes." "Publish it. We'll revise it later." So that's what they did. But they still are now official publications of the Geological Survey, and sometimes I see them quoted all over the world. [laughter]

But his point was that there was no excuse for ad hocing in a scientific organization the way they were doing, that there was no reason to have a backlog of publications. It was a matter of priorities, so he got the things done.

Lage: It must have forced some rethinking on the techniques at the same time.

Dawdy: Yes. In fact, all the techniques manuals were rewritten. It was very good, Luna's forcing them to get them published. Because these techniques were developed at the district levels, there might be six different techniques manuals in different districts. When there had to be one published, the branches had to take over and determine what should be published. So they assigned some of their better people to give a synthesis of these things and put out the techniques manuals. Yes, it was a big step forward to get that down and get it coordinated. But as usual, it created consternation.

Lage: What about research publications and control over quality?

Dawdy: The GS always had the review process, colleague review, and Luna strengthened and stressed that. But every research chief was expected to publish, and every paper that you published had to get colleague review, and you had to answer all the criticisms. That procedure was in effect before Luna came in, as I said.

The survey has always had publications of authors; most federal agencies have publications of the agency. So it's the U.S. Corps of Engineers, the Sacramento District, the San Francisco District, but it doesn't say who wrote it, whereas the U.S. Geological Survey always had "by So-and-so, U.S. Geological Survey," so that the author was responsible in the survey.

But his work was always reviewed and had to go through a fairly rigid review process. First colleague reviews; then these were sent in with the paper to a regional office or district office or whatever. It went through the district office to the regional office. The research papers went straight to the regional office, who then would check it over to see whether, in fact, the author met the arguments of the reviewers. And then it was sent to Washington and reviewed for "policy," whatever that review was, but also to make sure that there was an adequate technical content to the paper.

Review of Policy Statements in Research Papers

Lage: Did the policy review come into play often? It seems to me in some of my discussion with Luna, he implied he'd publish anything that was good research. What was the policy review?

Dawdy: This usually applied to the reports that came out of the district, rather than from the research. But in research, even, if you came up with a conclusion where you used field data and you then came to some statement about what should be done about a problem, this might or might not be--

Lage: It might conflict with the Bureau of Reclamation.

Dawdy: Well, it might conflict with what someone else in the organization thought, as far as that goes.

Lage: So what would happen then?

Dawdy: If it were a policy statement, it would either be watered down or eliminated. For example, I can remember writing a letter to a cooperator when I was assistant district chief, in which I was supposed to be telling about the progress on a research project being done within the district, where we were collecting a lot of data to study the effect of urbanization in San Diego County. I put in some interpretive reports that essentially said the data so far had shown such and such.

This went out to the district chief, and he wouldn't sign the thing. I was doing it for his signature. He bounced it and he said, "You're coming to a conclusion. You can't do that. You haven't been reviewed. We don't know that it's correct," all this sort of stuff.

Well, that's the sort of thing they look for in Washington, if you were coming up with a conclusion that wasn't based on the facts and evidence. Or if you were coming to a conclusion about something that wasn't part of what you were supposed to be doing, then they would bounce it and have you eliminate it. It never happened to me, but I do know that there were other people who got into trouble on policy matters, particularly when they were interagency things.

I know there was a study on the reservation in Arizona, where because of the political implications of the clearing of piñon and juniper and its effect on hydrology, that this paper, which was a research project on precisely that matter, kept getting bounced around because the authors kept wanting to make conclusions about what should be done rather than what happened when we did it. In fact, they never could understand why Washington was bouncing their papers. I don't know if we ever published that paper that kept getting bounced.

Lage: You seem to see this as legitimate. It was where the researcher was stepping beyond his bounds some?

Dawdy: Oh, yes, it's legitimate, sure. But the problem is that, for instance, let's say we're working on Kesterson, and the conclusion of the researcher is that it's the drain water that is creating the problem, therefore the solution is get rid of the drain water. Well, that gets rid of the irrigation. So therefore we should retire lands from irrigation. That may be a perfectly valid conclusion, but it doesn't have anything to do with his research. That's a decision for someone else to make besides him, someone in Washington to make, not the guy in the field.

Lage: So his job is just to say the problem is the drain water?

Dawdy: To identify what the elements are, where they come from, and what will happen if they go somewhere. The other implications of that at that particular time were beyond the secretarial level. They were at the political level.

Lage: Did Luna make any change in this kind of thing during his tenure?

Dawdy: No, I don't think so. That's--

Lage: That's pretty standard.

Dawdy: --pretty standard, yes.

Political Pressures on Research

Dawdy: I think that one of the differences today is that the politics have been pushed further down in the organization, that there's more political influence on the program of the Water Resources Division, even at the research level. I think this was evident in the Kesterson--

Lage: You mean congressional pressures?

Dawdy: Congressional and whoever it is that influences the secretary of the Interior. Probably the Bureau of Reclamation, who is influenced by their water users. The water users go to the bureau, the bureau goes to the secretary, and the secretary comes to the survey, and the director [of the survey] goes to the chief hydrologist, who then makes decisions about the program down at the field level.

Lage: Would this be in choice of what to study, or in publication of what has been studied?

Dawdy: Pretty much choice of what to do, in other words influencing the program. The sort of thing that I was saying, where the chief hydrologist will react by saying, "Yes, we can do that, and we will do that." And then some guy out in the field has to do it. Although there was, I'm sure, some of that which Luna was in. If he could see he could get some money from somebody to do something he wanted to do, I'm sure he would take the money.

I know Pecora started a program within the Geologic Division called the heavy metals program because Lyndon Johnson was having a gold crisis during the Vietnam War. Pecora was at some sort of an awards thing at the White House and happened to talk to the president who, when he heard he was director of the Geological Survey, said something about gold, and Pecora said, "Sure, we'll find gold for you." [laughter] So with that, he got \$25 million, or whatever it was, and he set up a program to go find gold. I don't think they found it.

Lage: Well, he got some gold for the agency. [laughter]

Dawdy: Yes, that's right. I think that's the main gold he found, because there were all sorts of weird programs developed. One of them was analyzing all of the sands along the California coast to see how much gold was in the beach sands.

Lage: How much gold had washed down through the delta?

Dawdy: Through the ages, yes. Apparently, there is a lot of very fine gold. In fact, during the Depression, out on the beach in San Francisco, a lot of people set up rockers and mined the beaches for gold in the thirties. So there is gold.

Lage: With some success?

Dawdy: Oh, yes.

Lage: Enough to make it worthwhile?

Dawdy: You didn't have to get much gold in those days. [laughs] Almost anything was a success.

Leopold's Treatment after Resignation as Chief Hydrologist

Lage: Another thing that's maybe a small matter, but Luna mentioned something about things that he wasn't aware of that were creating

ill will, and he thought you might be aware of them. The thing he mentioned was a protective secretary.

Dawdy: Oh, yes.

Lage: I don't know how important that is.

Dawdy: I told him about that. I, and probably others too, would go in to see Luna, and his secretary was very protective of Luna. She also wanted to impress, I guess, on us, just how important he was, so she would have us sit and cool our heels while Luna was not aware that we were being made to wait an hour or two. I just got up and walked out. I wasn't going to do that. She, I'm sure, antagonized a lot of people by her protectiveness. But that was a pretty minor thing, although I'm sure people thought it was Luna who was doing that, that it was his sense of self-importance that was doing it, where it was her sense of his self-importance that was doing it. [laughter]

Lage: Or her own. Her own power.

Dawdy: Yes, her own power too.

The other thing was when he was removed--you mentioned this--he moved to an office on, let's say the Water Resources Division was on the third floor and he was put in an office on the fourth floor. The order was essentially given that no one was to speak to him.

Lage: How did that come about? How can an order come down like that?

Dawdy: It wasn't written. It was just that it was let known by the chief that it wouldn't be appreciated if people were seen talking to Luna too much, because he was afraid that Luna might start a counter-revolution or something, I guess. So there were just a very few people who continued seeing Luna. He was isolated from the organization.

Then, after a couple of years of that, he moved out to Menlo Park, and in fact occupied the room right next to me with just a chest-high partition between us. Our desks were back to back, and his phone was right there and my phone was right there, so I heard him. When everyone found out that he had left Washington and moved to Menlo Park, they assumed that he was leaving the survey, so they started phoning him and offering him positions. The first several days that he was in that office, he was turning down positions. It seems to me that as soon as he stepped down, he came out here, and then he moved back to that floor, that office on the fourth floor.

Lage: Were you out here when he stepped down?

Dawdy: Yes.

Lage: So you didn't see him so much in Washington.

Dawdy: Well, yes, quite a bit, because I was commuting to Washington. I was back and forth all the time.

Lage: Is that a very unusual thing, this kind of isolation in the government service?

Dawdy: Very unusual. It was sort of a crime, I think. They should have used him as a resource. I'm sure Luna wanted to get back into research and he wanted to do his thing, but they should also have seen to it that he helped on the guidance of the organization.

Lage: He did not describe himself as being removed. He described that he'd been there ten years, and--

Dawdy: When he came in, he said he was going to stay for three years, then he was going to stay for five years, then he was going to stay for seven years. I think what really caused him to step down-- Yes, he wasn't removed, but the reason he finally stepped down was because Pecora came in, and he and Pecora were not what you would call copacetic. They didn't see eye to eye on how to run things. Pecora was very political.

Lage: And where did Pecora come from?

Dawdy: He was the chief geologist.

Lage: The chief geologist.

Dawdy: He moved up to be the director.

Lage: And was he a scientist himself?

Dawdy: Oh, yes.

Lage: But he was political.

Dawdy: Very political. And he and Luna apparently clashed when he was chief geologist and Luna was chief hydrologist, although I don't know anything about that. That was only a rumor that I heard later. But Luna didn't feel that he could work under Pecora very effectively, and I don't think Pecora thought Luna could work under him very effectively, so it was a mutual parting of the ways. When this became apparent was when this meeting in Columbus took place,

and Roy Hendricks started playing politics to become the next chief. I don't think Luna had any influence over who replaced him as chief hydrologist.

Lage: Is that a usual thing?

Dawdy: No, that's not very usual. Usually the person at least has some influence in recommending his successor because he knows the people in his organization better than anyone else, or should.

The Maverick Herb Skibitzke

Lage: Was there any resentment of these wonderful research trips or river trips and trips to Alaska and that kind of thing? He mentioned that Herb Skibitzke got a lot of flak also.

Dawdy: Oh, yes. That's a little different. Herb was just a different sort of a person. Herb, you should probably interview him on his reactions to Luna, too. Herb was the type who made up his own rules as he went along, so therefore if there was anyone who was bureaucratic in the organization, he would have a run-in with Herb, or Herb would have a run-in with him.

I remember someone telling about an apocryphal trip of Herb's one time, where he took off in his own personal airplane and flew to somewhere in Iowa, caught a train into Chicago, caught a commercial flight to New York, and rented a car and drove to Washington, D.C., and then somehow got back to Iowa to pick up his airplane and fly back. He turned in a travel authorization; his trip was to go to Washington, D.C. [laughter]

This was apparently one of the historical documents that was floating around Washington forever, trying to figure out how in the world to pay Herb his travel expenses for his trip. And of course, he had a book of transportation tickets, so anything that was public transport, he could pay for with these things. But he still had to be reimbursed for the part where he was off flying around and wandering around.

There was a similar trip to Africa one time that he went on with a guy by the name of Russ Brown, who was stationed in Phoenix for a while. He was also in Washington in the Groundwater Branch. Russ Brown went on this trip to Africa with Herb. And they flew over there. Herb decided, well, since we're in Chad, we should go to Rwanda or something, so he jumped on an airplane and flew over there, and then, "Gee, the guy that really knows about this is in

Rome, so let's go up to Rome," so they went to Rome. They went all over like that, all over Europe and Africa, and then finally got back to the United States. They turned in their travel vouchers. Years later, Russ Brown was still complaining he hadn't been paid yet for that trip. [laughter]

Herb, of course, did this every trip, so he had a big stack of these things in Washington that people were trying to figure out-- people who were trying to make Herb obey the rules. Herb was always in trouble with someone over something, but Herb also had some very good friends. His family was an old family in Phoenix, so he grew up with politicians who became senators and congressmen in Arizona. They had gone to school together and his father had gone to school with them, or something like that. So he had direct access to the congressmen and the senators, and ended up using that influence at times.

Lage: That wasn't appreciated--

Dawdy: It wasn't appreciated by some of the people that were involved in the organization.

Lage: Now, Luna seems to have supported him.

Dawdy: Oh, yes, very much so. So did the secretary of the Interior and so did the director, because Herb did things for people. Herb had his own private air force, which bugged a lot of people. He went out and got military surplus airplanes and trained all his people to be pilots. It's very funny because as I remember, Herb got a complaint for sex discrimination against him. There was a woman who was a secretary from one of the offices in Phoenix that got cut out because the guy was very dissatisfied with her work. Herb found a position for her doing very routine things in his office. Herb at that time was getting all of his people to get their pilot's license because he had the feeling or the theory that if he was somewhere a thousand miles away and he wanted something done immediately--

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Dawdy: --he needed someone in his office to fly. A couple of the people he hired were already hot-shot pilots. Two of them were two women who were pioneers in aviation. He hired as his chief technician a guy who had been part of the Blue Angels for the Marine Corps, so he was a hot-shot pilot. He owned a crop-dusting plane. But everyone else in addition, he made them take flying lessons so that they could get a pilot's license and fly. And he had three airplanes and helicopters and all sorts of things there that were

available for these people to do things--official things, for the U.S. Geological Survey.

Lage: Was he in a special office?

Dawdy: No, this was just his research project. His research project was-- . His philosophy on research was, the advice he gave to me was, "The way you do it is you always overspend. What they do then is they write you a letter reprimanding you for overspending your budget. The next year, they start you with the amount you overspent." So each year, you determine how much you want your project to grow, and you overspend by that amount. So his was a project that just kept growing. Of course, the people who he upset, I guess, never caught on to what he was doing, but they complained, so he had all sorts of reprimands and he was always in trouble with people in Washington, but he always went on obliviously doing what he thought had to be done.

So he ended up being the person who, when the secretary of the Interior wanted a tour, we had an Interior airplane and he could fly around anywhere with a pilot who knew the area and knew geology and all that sort of stuff. So when the director wanted to do something like that, he'd call on Herb, and Herb was willing to drop everything and go. And similarly, Luna used Herb's facility quite a bit. In fact, Luna took lessons from Herb, I think. I think that's where he learned to fly.

Lage: Yes, I think so.

Dawdy: In fact, I was there when he was getting some of his lessons, and I was one of Luna's first passengers.

Lage: A brave man.

Dawdy: Oh, I figured that he could take care of himself. I flew a lot with Herb. In fact, we were the crew that went out looking for Luna and the crew when John Miller got the bubonic plague and died in Boston.

Lage: Oh, way back then?

Dawdy: Yes. In fact, that may have been 1967. It may have been--

Lage: So you went out and found Luna in the field?

Dawdy: We went looking for him. We didn't find him. He was picked up by the Colorado Highway Patrol. There was a general all-points bulletin out for this whole group of people who had been in the field together.

Lage: Fearful that they also had bubonic plague?

Dawdy: They wanted them to get to the doctor and find out whether they had it, see whether they had any symptoms and to make sure that they were aware that they'd been exposed, because Miller had died and all these other people were missing. They all had gone out in the field, and none of them were anywhere. We didn't have any itinerary for anyone. So Herb and I flew around the Navajo Reservation looking for them. It turned out that the crew we were looking for had parked their truck under a tree so we couldn't see them. [laughter] We flew over them a couple of times, but we did leave word at Chinle that they were exposed, and they got to the doctor.

Lage: And did anyone else get exposed to it?

Dawdy: No. Well, they were all exposed, but nobody else got it. John Miller was the only one.

In Summary

Lage: Two more questions, to wind up today. Did you have any sense of how being a member of the distinguished Leopold family might have affected either Luna himself in this situation, or people's reaction to him?

Dawdy: What do you mean?

Lage: Did the fact that he came from such a distinguished family--

Dawdy: Oh, yes, everyone was aware of that, sure.

Lage: It sort of might have set him apart.

Dawdy: It certainly made him a natural leader. I'm sure it had an effect on him. He was very sure of himself and did things the way Luna wanted to, but other than the fact that I was aware and I guess others were aware also, I don't think that made much difference in what he did and how he did it. But everyone was aware of who his father was and who his brother and sister were. His sister, of course, was in the Geological Survey. But other than that, I didn't see any great influence myself, from my point of view.

Lage: This is a broad question, but as a wrap-up to this discussion, would you in a nutshell be able to describe Luna's importance in the science of hydrology?

Dawdy: Well, certainly he's been instrumental in quantitative geomorphology and in pushing the science of sediment transport, but more than that, I think his major impact came in his role in the Geological Survey. He started the real research program in the survey, which has really contributed to hydrology. He started essentially the Office of Water Resources Research and the emphasis on hydrology in the universities. That has now certainly a life of its own. It's hardly even recognized that that's where it came from. He saw an immediate need to set up hydrology as a separate study in the university, and he helped set up the hydrology program at the University of Arizona. He pushed for the Office of Water Resources and Research in Interior, which became OWRT and then died and was reborn again. But it was a means for getting research money into hydrology in the universities. So I think all of those things together.

His own personal impact in his research has been his wide-ranging interest in process-oriented problems and his interest in the application of these things to public policy matters. He's been very involved in trying to introduce science into decision-making, and involved in all sorts of environmental matters because of that.

Lage: Well, that's a very good summary. Thank you so much.

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